

Quasi-experiments in political behaviour



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To my wife Roxanne

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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I confirm that Chapter 2 was jointly authored with Janne Tukiainen and that I contributed 75% of this work.

Statement on Published Work

I confirm that Chapter 4 has been published as a peer-reviewed journal article in *Electoral Studies* (Dunaïski, 2021).

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Abstract

Political science has long sought to understand how citizens form their political identities, values and behaviours. However, robust causal evidence on how policy interventions can shape the political socialisation process remains limited. The three papers that make up this thesis investigate how citizens - and young citizens in particular - change their political attitudes and behaviours in response to large-scale policy interventions, specifically: income transparency, enfranchisement and compulsory voting.

In the first paper, I take advantage of a quasi-experiment in Finland to study whether income transparency - the public release of citizens' income information - affects support for redistribution. Using survey data and a before-and-after research design, I show that income transparency leaves public support for redistribution largely unchanged, but that young people increase their support for redistribution in response to the intervention. This suggests that redistributive preferences are rooted in more stable, underlying ideologies, that are difficult to alter once they are formed in early adulthood.

In the second paper, I leverage a quasi-experiment in Germany to study whether enfranchisement improves citizens' political maturity. Using survey data and a difference-in-differences approach, I show that enfranchising 16-year-olds can equalise prior differences in political maturity between underage and adult youth. This suggests that political maturity should be understood not just as a precondition, but also as an outcome of the right to vote.

In the third paper, I take advantage of a quasi-experiment in Brazil to investigate whether compulsory voting instils voting habits in young people. Using administrative data and a regression discontinuity design, I show that voting fails to be habit-forming when it is compulsory. This finding clarifies the scope conditions of prior research on voting habits, as it runs counter to available evidence from voluntary voting systems.

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Chapter 1

Introduction

How much do you care about politics? Which party do you vote for? Do you vote at all? For many citizens, the answers to these questions crystallise during early adulthood and remain relatively constant thereafter. As early as the 1950s, political scientists drew attention to the importance of studying young people's political socialisation, as they observed that, later in life, individuals' political attitudes and behaviours tend to become characterised by continuity and regularity (Hyman, 1959). Several decades of research on political socialisation have since shown that citizens do indeed develop relatively stable political attitudes and behaviours during their adolescence and early adulthood, the so-called "impressionable years" (Bartels and Jackman, 2014; Neundorf and Smets, 2017; Sears and Valentino, 1997). While there is still no consensus on the precise age bracket that defines the impressionable years, many studies focus on the years 17-25 (Neundorf and Smets, 2017). These studies typically find that, during the impressionable years, individuals' political attitudes and behaviours are malleable by socialising agents (e.g. parents, teachers, or peers) and salient events (e.g. economic shocks or elections) (Neundorf and Smets, 2017). Later in life, however, political attitudes and behaviours can only be shifted temporarily, if at all (Margalit, 2013).

Which factors shape how young people think and act politically? The literature has identified several important socialising agents, from parents and teachers, to peers and social media (Neundorf and Smets, 2017). In contrast, the political context in which young people grow up has received relatively little attention. Several studies have examined the influence of early electoral experiences on political engagement later in life (Franklin and Hobolt, 2011; Franklin et al., 2004; Plutzer, 2002). In addition, a few studies have analysed at how early life exposure to economic shocks or political scandals affects attitudes and behaviours later in life (Dinas, 2013; Finseraas, 2017; Giuliano and Spilimbergo, 2014; Roth and Wohlfart,

2018). However, we still know relatively little about how specific policy interventions can shape the political socialisation process.

This thesis examines how large-scale policy interventions, specifically income transparency, enfranchisement, and compulsory voting, affect citizens' political attitudes and behaviours. From a public policy perspective this is important, because unlike political scandals or parental influence, the interventions studied here fall within the traditional remit of government action. Therefore, the findings can be used to inform on-going policy debates. For example, by studying the effects of income transparency on public support for redistribution, the first paper speaks to the on-going debate about how to counteract the trend towards growing economic inequality that has crept across most of the industrialised world (Piketty and Saez, 2014). By studying the consequences of lowering the voting age to 16, the second paper speaks to the debate about how to improve the political engagement of young people, who tend to be much less involved in formal politics than older generations (Eichhorn and Bergh, 2020). Finally, by studying the consequences of compulsory voting, the third paper speaks to the debate about how to counteract low and unequal turnout that afflicts many advanced democracies (Lijphart, 1997).

1.1 Contributions

Each paper in this thesis makes specific contributions to the fields of political behaviour and political economy. In the first paper, I study how income transparency - a policy intervention that increases citizens' exposure to information about economic inequality - affects public support for redistribution. Previous studies in this area have primarily relied on survey experiments, where subjects are exposed to customised information about income inequality or their position in the income distribution (Cruces et al., 2013; Kuziemko et al., 2015). However, such information treatments rarely occur in the real world, so we do not know whether previous findings apply outside the experimental setting. I find that income transparency leaves support for redistribution largely unaffected, which indicates that shifting redistributive preferences may be more difficult to achieve via real-world policy interventions.

In the second paper, I study whether the enfranchisement of 16 year-olds affects their political maturity - measured by their political interest, efficacy, willingness to vote, and attitudinal consistency. Previous studies on voting at 16 have attempted to show that young people at this age are not politically mature enough to vote by employing data on *disenfranchised* 16 year-olds or by extrapolating from data on enfranchised over-18 year-olds (Chan and Clayton, 2006; McAllister, 2014). This is problematic, however, as the right to vote may

itself influence the political maturity of previously disenfranchised groups. In line with this reasoning, I find that enfranchisement can equalise prior political maturity differences between underage and adult youth, which suggests that political maturity should be understood not just as a precondition but also as an outcome of the right to vote.

In the third paper, I study whether compulsory voting can instil voting habits in young people. Using data from voluntary voting systems such as the US or the UK, previous studies have found that voting in one election increases one's propensity to vote in the future, which they interpret as habit formation (Coppock and Green, 2016; Dinas, 2012; Fujiwara et al., 2016; Meredith, 2009). In contrast, I find no evidence that voting is habit-forming when it is made compulsory. Instead, the evidence points to a first-time compulsory voting boost, which gradually dissipates as voters grow older. The paper therefore clarifies the scope conditions of prior research on voting habits and shows that previous findings do not necessarily generalise to contexts where voting is compulsory - as is the case in around one quarter of all democracies worldwide.

1.2 Methodological approach

Studying the impact of policy interventions on individuals' attitudes and behaviours poses significant empirical challenges. In particular, establishing causality is not a trivial task. The three papers in this thesis share a common methodological approach in that they all employ econometric methods, which allow us to draw causal conclusions about the impact of policy interventions on individuals' attitudes and behaviours. In this respect, all three papers build on the recent "credibility revolution" in the social sciences, which has seen the randomised controlled trial elevated to the "gold standard" against which to assess research designs (Angrist and Pischke, 2010). Randomised trials can be expensive, time consuming, and may not always be practical or ethical to implement. Fortunately, in many instances, researchers can take advantage of human institutions or forces of nature to construct informative natural- or quasi-experiments that come close to the gold standard of the randomised trial (Angrist and Pischke, 2010).

Natural experiments occur in situations where there is random assignment of a treatment via a randomisation device (e.g. the Vietnam draft lottery), but where, in contrast to the randomised trial, the assignment is not under the control of the researcher (Gerber and Green, 2012).¹ Quasi-experiments in turn occur in situations where "as-if random" processes (e.g.

¹Some definitions of the term natural experiment do not require random treatment assignment (e.g. Dunning 2012).

near-victories and near-defeats in elections) cause different places, groups, or individuals to receive different treatments (Gerber and Green, 2012). Because they do not employ an explicit randomisation device, the causal inferences that quasi-experiments support are subject to greater uncertainty and typically require additional supporting evidence (Gerber and Green, 2012).

In all three papers, I use such quasi-experimental methods to isolate the causal effect of policy interventions on citizens' political attitudes and behaviours. The first paper takes advantage of the fact that in Finland, the public release of citizens' income information - the so-called tax day - coincides with the implementation period of the European Social Survey (ESS). Whether ESS respondents were interviewed shortly before or after the tax day can be considered as-if random, so a comparison between the two groups can offer insights into the causal effect of income transparency. The second paper leverages the fact that in Germany, different sub-national states lowered the voting age to 16 at different times over the last decades. By comparing survey respondents in different states before and after the voting age reform, we can learn something about the causal effects of enfranchisement. Finally, the third paper takes advantage of the fact that in Brazil, voting is voluntary at age 16 and then becomes compulsory at age 18. By comparing turnout patterns between citizens who were just old enough and just too young to be eligible for compulsory voting, we can establish whether making elections compulsory instils voting habits in young people.

1.3 Case selection

The country case selection for the three papers was primarily determined by data availability and each country's unique institutional context, which enabled the quasi-experimental analyses.

The regression discontinuity approach used in Chapter 4 requires a large data set, as it focuses on a narrow sub-set of citizens who turn 18 shortly before and after the election (and discards all observations further away from the voting age discontinuity). Fortunately, Brazil's progressive open data legislation means that the complete administrative voter records are available to researchers. Furthermore, with an electorate of around 170 million, Brazil is also the most populous country in the world to use compulsory voting. Taken together, this lends considerable statistical power to the analysis of the administrative voter records.

The selection of Germany as a case study for Chapter 3 was guided by similar considerations. Finding a suitable setting to investigate the impact of enfranchising 16 year-olds on their political attitudes and behaviours is challenging. Not only because very few countries have lowered the voting age to 16 (Eichhorn and Bergh, 2020), but also because most nationally representative surveys restrict their samples to those aged 18 and above. Germany is unique in that its (sub-national) states lowered the voting age to 16 in a staggered process starting in the late 1990s. Germany also has long-running youth surveys, which include questions on politics and capture the crucial age group of newly enfranchised 16 year-olds.

In Chapter 2 I use a before-and-after type research design to study the effects of income transparency on support for redistribution. This research design relies on the fact that Finland's tax day coincides with the implementation period of the ESS. However, there are additional reasons why Finland presents an ideal case study. Income transparency policies are being increasingly used by companies and local governments to reduce gender and racial pay gaps (Baker et al., 2019; Cooney, 2018) and tackle excessive executive pay (Mas, 2017). But there are only very few countries - most of them in Scandinavia - that have elevated income transparency to the level of state policy. Finland shares with its Scandinavian neighbours a long tradition of making citizens' tax records publicly available (Perez-Truglia, 2020). However, Finland is unique in that it has turned the annual release of citizens' income information into a "public ritual of comparison", where for a few days each year the issue of income inequality is at the centre of public debate and media attention (Barry, 2018). Finland is also one of the most equal societies in the industrialised world, which makes it a particularly "hard case" to detect effects of income transparency on citizens' attitudes towards inequality.

Chapter 2

Does income transparency affect support for redistribution? Evidence from Finland's tax day

Abstract: The disconnect between rising inequality and lack of support for redistribution in Western democracies raises the question of whether specific policy interventions can shift demand for redistribution. This paper examines whether income transparency - the public release of citizens' income information - affects support for redistribution. We take advantage of a quasi-experiment in Finland, where every year on the so-called tax day, the authorities release income information on Finland's top-earners to the public. To identify the causal effect of the tax day we compare respondents who took part in the European Social Survey shortly before and after the event. We find that the tax day increases income comparisons and perceptions that earnings of the top 10% are unfair, but that public support for redistribution remains largely unaffected. A notable exception are top-earners, who decrease their support for redistribution, and young people, who increase their support for redistribution. Our results highlight the scope conditions of previous survey- and field experiments, and suggest that increasing exposure to inequality through a real-world policy, rather than experimental treatments, may trigger only marginal changes in support for redistribution.

Keywords: Income transparency, inequality, redistribution, taxes

JEL Codes: D63, D80, H20

2.1 Introduction

“Finland is unusual, even among the Nordic states, in turning its release of personal tax data into a public ritual of comparison. Though some complain that the tradition is an invasion of privacy, most say it has helped the country resist the trend toward growing inequality that has crept across of the rest of Europe.”¹

New York Times, 1 Nov 2018

Income inequality has increased substantially in most industrialised democracies over the past decades (OECD, 2011). Figure 2.1 below shows that between 1980 and 2019, the share of total pre-tax income going to the top 10% of US adults increased from 33% to 45%, while the share going to the bottom 50% decreased from 20% to 13%. Similar trends can be observed in Europe, including in Finland. The canonical political economy model predicts that governments will face greater pressure to redistribute income as inequality increases and the distance between the median voters’ income and the mean income in society grows (Meltzer and Richard, 1981).² Yet, contrary to the model’s predictions, rising inequality has not led to an increase in public support for redistribution (Ashok et al., 2015; McCall et al., 2017).³ This disconnect has triggered a large amount of research into the factors that might suppress demand for redistribution, including lack of information (Alesina et al., 2018; Cruces et al., 2013; Hvidberg et al., 2020; Kuziemko et al., 2015), political ideology (Alesina and Fuchs-Schuendeln, 2007), fairness beliefs (Alesina and Angeletos, 2005) and ethnic heterogeneity (Alesina et al., 1999, 2001; Dahlberg et al., 2012). However, we still know very little about whether and how specific policy interventions can shift support for redistribution (Trump, 2018).⁴ Several recent survey- and field experiments (Condon and Wichowsky, 2020; Cruces et al., 2013; Dietze and Craig, 2020; Fehr et al., 2019; Karadja et al., 2017; Kuziemko et al., 2015; Sands, 2017; Thal, 2020) try to address the question of what it takes

¹<https://www.nytimes.com/2018/11/01/world/europe/finland-national-jealousy-day.html>

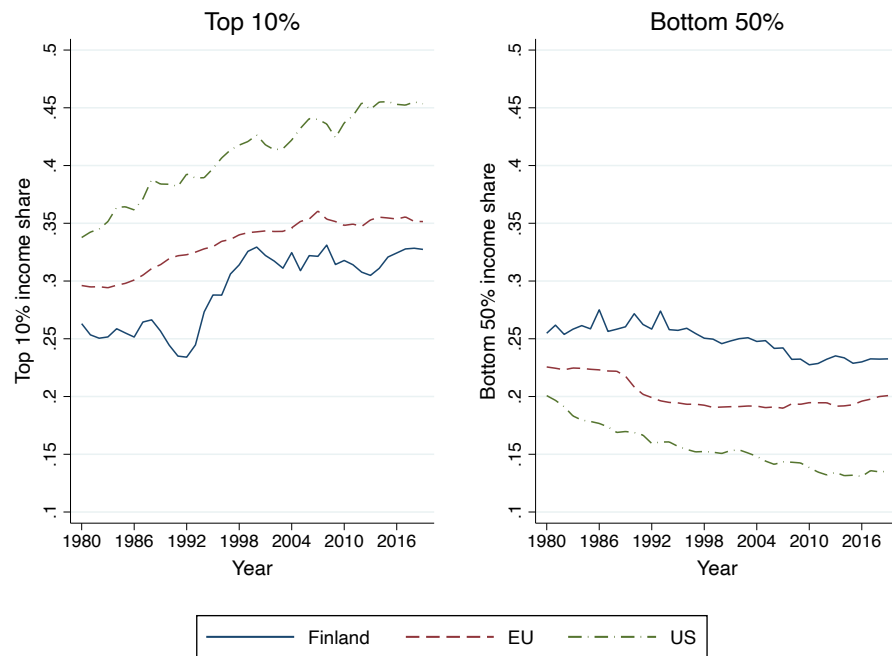
²Most political economists agree that the Meltzer-Richard model has limited explanatory power and that it makes unreasonable assumptions (e.g. that voters have perfect information) (Bredemeier, 2014). However, the model continues to be widely used for its analytical tractability, as it sets a clear benchmark for assessing the value of alternative theories (see Cavaille 2020).

³Survey evidence from several industrialised democracies suggests that support for redistribution has remained flat or even decreased, depending on the measure used (Ashok et al., 2015; McCall et al., 2017). Redistribution refers to the process of taking material goods from those who need it least, and giving it to those who need it most (Cavaille, 2020).

⁴The comparative welfare state literature has long been interested in how welfare spending affects public attitudes towards the welfare state (see Busemeyer et al. 2021 for a review). However, the focus has primarily been on explaining long-run trends and cross-country differences in public opinion, rather than identifying the causal effect of specific policy interventions.

to shift demand for redistribution by manipulating subjects' exposure to information about inequality. While some find that subjects adjust their redistributive preferences when exposed to information about inequality (e.g. [Cruces et al. 2013](#)), others find that redistributive preferences remain largely unaffected (e.g. [Kuziemko et al. 2015](#)). A limitation of these studies is that the experimental manipulations are either customised information treatments that rarely occur in the real world ([Cruces et al., 2013](#); [Kuziemko et al., 2015](#)), or interventions in the field that are difficult to implement on a larger scale, such as randomising the presence of a poor person in a wealthy neighbourhood ([Sands 2017](#)).

Fig. 2.1 Income inequality in Finland, the EU and the US (1980-2019)



Data: World Inequality Database. *Note:* The graph shows the share of total pre-tax income going to the top 10% of adults (left panel) and the bottom 50% of adults (right panel) in Finland (solid), the EU (dashed) and the US (dash-dotted) for the years 1980-2019.

Our paper advances the literature by studying the effect of a real-world policy (income transparency) on support for redistribution. Income transparency – the public release of citizens' income information – has been promoted as an effective policy intervention to reduce gender and racial pay gaps ([Baker et al., 2019](#); [Cooney, 2018](#)), tackle excessive executive pay ([Mas, 2017](#)), and deter tax evasion ([Bø et al., 2015](#)). It remains unclear, however, whether income transparency can lead to shifts in public support for redistribution. Despite the lack of evidence, we have good reasons to expect an effect. Income transparency

is a policy that increases citizens' exposure to information about income inequality (Perez-Truglia, 2020), and previous survey- and field experiments suggest that such exposure can correct misperceptions about inequality (Hvidberg et al., 2020) and trigger greater demand for redistribution (Cruces et al., 2013; Sands and de Kadt, 2020).

We study the effect of income transparency on redistributive preferences in the context of Finland, where public support for redistribution is relatively high and income inequality is low compared to other industrialised democracies (see Figure 2.1). To isolate the causal effect of income transparency on citizens' attitudes, we take advantage of a quasi-experiment in Finland, where every year on the first working day of November, the tax authority releases the income information of everyone who earns more than €100,000 per year to the media. The so-called tax day (*Veropäivä*) triggers an annual media spectacle focused on Finland's top-earners, celebrities and potential tax dodgers (Barry, 2018). Given that the tax day takes place every year, we argue that the event primarily serves to increase the *salience* of inequality in the public debate, rather than providing citizens with much new information about income inequality in Finland.

We use media data from 2019 to show that the tax day coincides with a sharp spike in the salience of income inequality in the media. To estimate how the tax day affects citizens' attitudes, we compare respondents who took part in the 2002-2018 European Social Surveys (ESS) shortly before and after the event. We find that the tax day increases income comparisons and perceptions that earnings of the top 10% are unfair. The effects are strongest amongst below-median income earners. Despite these initial reactions, we find that the tax day leaves citizens' support for redistribution largely unaffected. We show that the overall null effect is precisely estimated and unlikely due to ceiling effects or the repeated nature of the tax day. However, the overall null effect also hides substantial heterogeneity. We find that individuals in the top income decile respond to the tax day by decreasing their support for redistribution, while individuals in the youngest age group (15-24 years) respond by increasing their support for redistribution.

We explore potential mechanisms behind these heterogeneous effects. One explanation is that the tax day suppresses demand for redistribution amongst the top income decile because it triggers a process of motivated reasoning aimed at justifying their privileged position in the income distribution, in line with recent sociological research on Finland's top-earners (Kantola, 2020; Kantola and Kuusela, 2019). The reaction amongst the youngest age group (15-24 years) in turn appears to be driven by the tax day's effect on (mis-)perceptions of relative income status, which we show is stronger amongst the youngest age group compared to older age groups. Taken together, our findings indicate that income transparency can

increase citizens' concern about income inequality, but may only marginally affect their support for government action to ameliorate inequality. While the egalitarian context and the repeated nature of the Finland's tax day may limit the generalisability of our findings, they imply that lack of exposure is not the key constraint preventing demand for redistribution from "keeping up" with rising inequality. Instead, redistributive preferences appear to be rooted in more stable, underlying ideologies, and may be difficult to alter once they are formed in adolescence and early adulthood.

Our results have several important implications. First, they highlight the scope conditions of previous survey- and field experiments (Condon and Wichowsky, 2020; Sands, 2017; Sands and de Kadt, 2020) which have shown that subjects' support for redistribution can be manipulated by exposing them to information about inequality. Our results indicate that triggering such a response may be more difficult to achieve via real-world policy interventions. Second, lack of exposure to inequality (e.g. due to residential or educational segregation) is frequently put forward as an explanation for why demand for redistribution has not kept up with growing inequality (Condon and Wichowsky, 2020). Our results suggest that policy interventions aimed at increasing cross-class exposure may not necessarily be sufficient to address this mismatch. Third, our findings alleviate a common concern expressed by critics of income transparency, which is that such policies could lead to a "populist backlash" (Mas, 2017). In Finland, we find no evidence that income transparency triggers a left- or right-ward shift in public opinion.

Related literature - Our paper relates to several strands of literature. First, we contribute to the literature on income transparency by studying, for the first time, its effect on political attitudes. Previous studies have shown that the release of income information can affect individuals' job satisfaction (Card et al., 2012), job retention (Mas, 2017), job performance (Blanes-i Vidal and Nossol, 2011; Cullen and Perez-Truglia, 2018), salary negotiations (Baker et al., 2019), and tax compliance (Bø et al., 2015; Hasegawa et al., 2012). Most closely related to our paper is a recent study by Perez-Truglia (2020), who finds that income transparency in Norway widened the gap in self-reported happiness between the rich and poor by 29% and increased the life satisfaction gap by 21%. We shift the focus to political outcomes and ask whether income transparency can affect individuals' support for redistribution.

Second, our paper speaks to the literature on redistributive preferences. We contribute to a growing body of experimental studies that attempt to manipulate subjects' support for redistribution by providing them with information about inequality (Brown-Iannuzzi et al., 2015; Condon and Wichowsky, 2020; Cruces et al., 2013; Fehr et al., 2019; Karadja

et al., 2017; Kuziemko et al., 2015; Thal, 2020).⁵ A limitation of this literature is that individuals' support for redistribution is typically manipulated via customised information treatments in survey experiments, which rarely occur in the real world. So it remains unclear whether previous findings apply outside the experimental setting. To overcome this challenge, two recent studies instead use field experiments that expose subjects to visible markers of inequality. Sands (2017) randomizes the presence of a visibly poor person in wealthy neighborhoods in Boston and finds that wealthy individuals become less supportive of redistribution as a result. Sands and de Kadt (2020) run a field experiment in South Africa where they randomize the presence of an expensive car in a poor neighborhood. They find that passersby who are exposed to an expensive car are more likely to sign a wealth tax petition. A challenge for these studies is that it may not be possible (or ethical) to implement the experimental manipulations on a larger scale, so the policy implications remain uncertain. We address the limitations of previous experimental work by studying the effect of a real-world policy (income transparency) on support for redistribution.

Third, our paper relates to research on political socialisation, which shows that individuals form stable political opinions during adolescence and early adulthood, the so-called "impressionable years" (Bartels and Jackman, 2014; Neundorff and Smets, 2017; Sears and Valentino, 1997). There is no consensus on the precise age bracket that defines the impressionable years, but studies typically focus on the years 17-25 (Neundorff and Smets, 2017). Most closely related to our paper is a recent correlational study by Roth and Wohlfart (2018), who use survey data from the US and Europe to show that individuals who experienced higher macro-level inequality during their impressionable years tend to be less supportive of redistribution.⁶ We contribute to this literature by providing first causal evidence that exposure to information about inequality during the impressionable years can trigger greater demand for redistribution, whilst older age groups remain largely unaffected.

Fourth, our paper speaks to political economy research which shows that exposure to media content can shape individuals' political beliefs and preferences (Dellavigna and Kaplan, 2007; Druckman and Parkin, 2005; Durante et al., 2019; Gerber et al., 2009; Hennighausen, 2015). Most closely related to our paper is a study by Hennighausen (2015) who uses quasi-experimental evidence from socialist East Germany to show that exposure to West German

⁵Kuziemko et al. (2015), Cruces et al. (2013), Karadja et al. (2017), and Fehr et al. (2019) provide objective income information treatments in survey experiments, with mixed results. Brown-Iannuzzi et al. (2015), Condon and Wichowsky (2020) and Thal (2020) in turn manipulate survey respondents' subjective social status, and find heterogeneous effects on redistributive preferences depending on respondents' objective income status.

⁶Several studies also examine whether experiencing a recession during the impressionable years affects support for redistribution, with mixed findings (Carreri and Teso, 2020; Giuliano and Spilimbergo, 2014; Neundorff and Soroka, 2018).

media increased beliefs that effort rather than luck is important in determining success in life. We contribute to this literature by showing that media coverage of Finland's tax day affects citizens' fairness beliefs and income comparisons, whilst leaving their redistributive preferences largely unchanged.

Finally, our paper relates to research in economics which shows that individuals care about their relative income.⁷ In a seminal contribution, [Easterlin \(1974\)](#) provides correlational evidence that relative income is a key determinant of subjective well-being. Similarly, [Luttmer \(2005\)](#) and [Ferrer-i Carbonell \(2005\)](#) find that, holding own income constant, subjective well-being decreases with the mean income of neighbors and other reference groups. Recent experimental research suggests that the effect of relative income on well-being is non-linear. [Kuziemko et al. \(2014\)](#) find that subjects are particularly averse to being ranked "last" in the income distribution, while [Fisman et al. \(2020\)](#) find that subjects are averse to "topmost" as well as "local" inequality. We study a policy intervention that exposes the incomes of a nation's top-earners, and find that this decreases subjective well-being at the bottom of the income distribution.

2.2 Background

Finland is a consolidated democracy with one of the most comprehensive welfare systems in the world ([Pesonen and Riihinen, 2002](#)). Support for redistribution in Finland is relatively high and economic inequality is low compared to other industrialised democracies. Figure 2.1 shows that, even though income inequality in Finland increased markedly since the early 1990s, it has done so at much lower levels than in the US and the rest of Europe. Support for redistribution in Finland is also higher, on average, than in the rest of Europe. For example, in the ESS data from 2002-18, Finnish respondents are significantly more likely to agree with the statement that the government should take measures to reduce differences in income levels, compared to respondents in the rest of the EU.⁸ As in many other Western democracies, the rich in Finland are less supportive of redistribution than the poor. In the Finnish ESS data from 2002-2018, the correlation between respondents' support for redistribution ("the government should take measures to reduce differences in income levels") and respondents'

⁷See [Boskin and Sheshinski \(1978\)](#) for a seminal theoretical contribution to the literature.

⁸Response options range from disagree strongly (1) to agree strongly (5). The mean response in Finland is 3.92 and the mean response in the rest of the EU is 3.86. The difference in means is relatively small but statistically significant ($t = -7.576, p < 0.001, n = 373,979$). Data are from all available ESS rounds between 2002 and 2018. Israel, Russia, Turkey, and Ukraine are excluded.

income rank (household income deciles) is negative ($\beta = -0.17$) and statistically significant ($p < 0.001$).

Income transparency has a long tradition in Finland and, in many parts of the country, municipal tax records have been publicly available as far back as the 1920s. From the 1960s until the late 1980s, ordinary citizens could purchase so-called tax calendars, which contained the income information of everyone in their municipality. In 2000, new legislation came into force, which allowed the media to purchase lists of individuals with the highest taxable (earned and capital) income in Finland.⁹ The tax lists, which include everyone with a pre-tax income of €100,000 or more in the previous tax year, are released to the media on the first working day of November of every year.¹⁰ The specific information released on the tax day includes the person's name, year of birth and province of residence, the total earned and capital income subject to taxation, the total amount of taxes and levies paid, and the total amount of tax refund. The timing of the tax day is unrelated to other important political events in Finland (e.g. national elections), and was chosen because the tax calendar ends in October. Table A1 in the appendix lists the exact dates for all tax days since 2000.

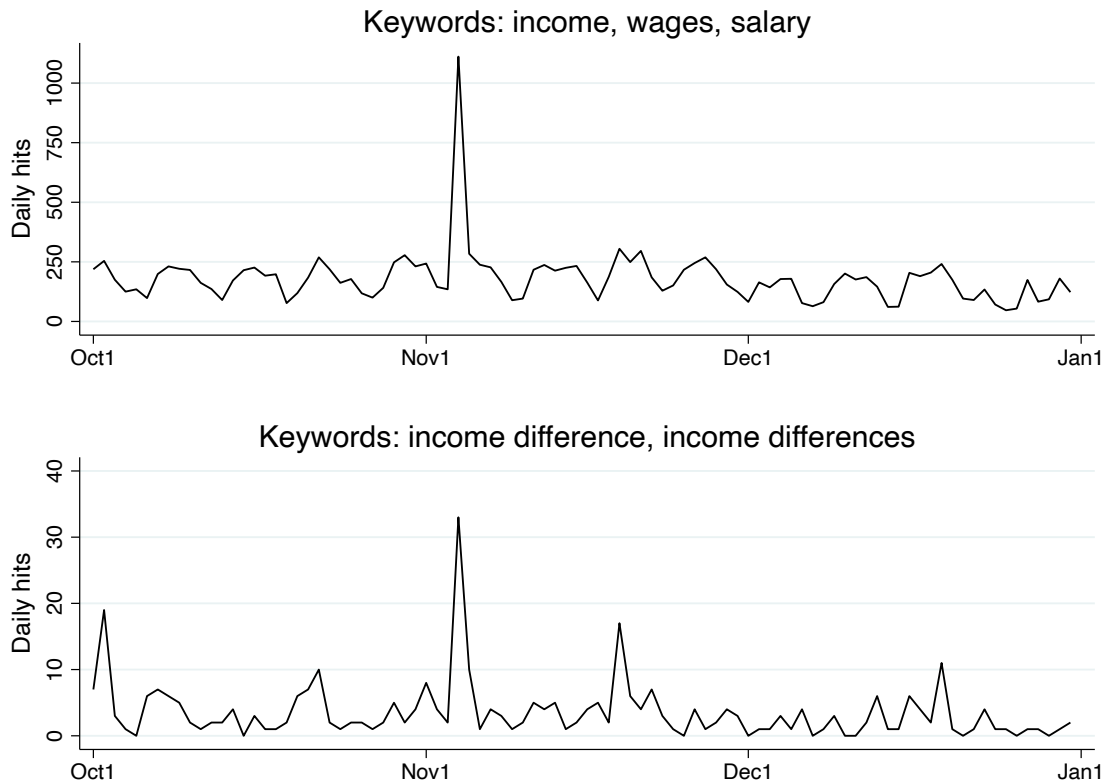
Ever since the law change in 2000, the tax day has become an annual media spectacle focused on Finland's top-earners, celebrities and potential tax dodgers (Barry, 2018). Historically, income transparency in Finland was justified as a means to ensure tax compliance (Lohiniva-Kerkelä, 2003). Today, the tax day is often justified as a means to encourage cross-class comparisons between the rich and poor, and the issues of economic inequality and redistribution feature prominently in the public debate (Barry, 2018; Yläjärvi, 2020). Several national newspapers such as *Helsingin Sanomat* and *Ilta-Sanomat* use the tax day to launch or update databases (*Verokone*) that allow readers to search for the names and incomes of Finland's top-earners.¹¹ The tax day is a highly salient event in Finland and one of the most important media events of the year (Barry, 2018). Figure 2.2 below uses data from 2019 to show that the tax day creates significant spikes in media coverage related to keywords such as salary, income, and inequality during the first few days of November. The spikes are large, but relatively short-lived. Figure A1 in the appendix shows that Google search queries related to the tax day follow the same pattern.

⁹An English translation of the law is available at: <https://finlex.fi/en/laki/kaannokset/1999/19991346>

¹⁰Since 2019, individuals can request to be removed from the list of top-earners that is shared with the media on the tax day. In 2019, around 200 top-earners had their information removed. In 2020, this rose to around 4,400 as the request could be submitted online via OmaVero. Requests have to be made separately each year.

¹¹Note that the income information of every Finnish citizen (regardless of their income) can be requested by phone or via customer terminals in local tax offices.

Fig. 2.2 Daily media hits for keywords related to the tax day (Oct–Dec 2019)



Data: LianaMonitor/VATT Institute for Economic Research. *Note:* In 2019, the tax day was on Monday, November 4th. The Finnish keywords for the top panel were “tulot”, “palkat”, “palkka”, and for the bottom panel the Finnish keywords were “tuloero”, “tuloerot”. LianaMonitor searches all Finnish-language news articles published online within a specified time period. This means that radio and television content is likely underrepresented relative to print media content.

2.3 Conceptual framework

In this section we briefly set out our expectations about what signals the tax day sends to Finnish citizens and how we expect them to respond to these signals.

2.3.1 The tax day increases the salience of inequality

Given that Finland’s tax day has taken place every year since 2000, we do not expect the event to provide the public with much new information about the objective level of income inequality in the country. Instead, we argue that the tax day and the resulting media coverage of Finland’s top-earners primarily serves to bring the issue of income inequality to the

attention of ordinary citizens. In other words, we expect the tax day to increase the *salience* of income inequality amongst the public.¹² We use the term *salience* to refer to the degree to which citizens engage with a political issue, in our case income inequality (Moniz and Wlezien, 2020).¹³ If the tax day increases the degree to which citizens engage with the issue of income inequality, we expect the event to increase perceptions that incomes at the top are unfairly high, especially amongst the less affluent.

H1a: *The tax day increases perceptions that the incomes of the top 10% are unfairly high, especially amongst the less affluent.*

The social psychology literature argues that people make sense of abstract concepts such as inequality by engaging in social comparisons, i.e. by comparing their own situation with that of other people (Tajfel, 1981; Tajfel and Turner, 1979). Income is only one dimension along which individuals can make such social comparisons (others are ethnicity, gender, or age), but income is considered one of the most important markers of social status in Western societies (Thal, 2020; Veblen, 1899). If the tax day increases the salience of income inequality amongst the public, we expect it to also trigger income comparisons between citizens. The media coverage of the tax day focuses primarily on individuals who earn more than €100,000 per year, as these top-earners are included in the tax list that is shared with the media. We expect the media focus on the super-rich to primarily trigger “upward” income comparisons amongst the public.¹⁴ Given that incomes of more than €100,000 per year are significantly higher than the incomes of those at the lower end of the highest (10th) income decile (€70,020 per year), we expect the tax day to trigger upward income comparisons amongst the affluent and less affluent alike.¹⁵

H2: *The tax day increases upward income comparisons, regardless of individuals’ income status.*

¹²In Figure 2.2 we provided some initial evidence that the tax day triggers a spike in the salience of income inequality in the Finnish media. However, we also expect the tax day to increase the salience of income inequality amongst ordinary citizens (not just in the media).

¹³Issue salience can be conceived as being a function of two factors: first, the importance an individual attaches to the issue and, second, the extent to which an individual perceives the issue to be a problem (Moniz and Wlezien, 2020).

¹⁴An individual engages in upward income comparisons when she compares her own income to those who earn more than her, rather than the same as her or less than her (Condon and Wichowsky, 2020).

¹⁵Estimated income deciles are from the 2018 ESS Finland.

2.3.2 Alternative signals

A crucial assumption in our conceptual framework is that the tax day increases the salience of income inequality amongst the public. However, there are at least two alternative signals that the tax day could send to Finnish citizens. First, that hard work pays off, and second, that the rich pay their fair share of taxes.

If the tax day primarily acts as a signal to citizens that hard work pays off (i.e. that income differences in Finland are justified to reward effort), we expect the tax day to *reduce* perceptions that the incomes of the top 10% are unfairly high. We would expect such a negative effect on unfairness perceptions regardless of individuals' own income status. This expectation builds on research in social psychology, which shows that individuals support higher levels of inequality than predicted by their own self-interest when they believe inequality to be the result of a fair process (Trump, 2020). In the Finnish context, the hard-work-pays-off hypothesis makes intuitive sense given that the national media regularly proclaim "capitalist heroes" on the tax day. In recent years, for example, several media outlets ran favourable tax day stories about the young millionaire owners of the Finnish gaming company *Supercell* (Barry, 2018).

H1b: *The tax day decreases perceptions that the incomes of the top 10% are unfairly high.*

Another plausible signal the tax day could send to citizens is that the rich pay their fair share of taxes (i.e. that the Finnish tax and welfare systems work as they should). This scenario is plausible because the list of top-earners shared with the media does not only report their taxable (earned and capital) income, but also reports the total amount of taxes paid by each individual on the list. If this is the main signal that the tax day sends to citizens, we expect the event to have no effect on perceptions that the incomes of the top 10% are unfairly high, as such a signal would favour the status quo.

H1c: *The tax day does not affect perceptions that the incomes of the top 10% are unfairly high.*

It is likely that the tax day sends different signals to different types of people, given that citizens will self-select into different media coverage of the tax day (e.g. Finnish tabloids tend to focus on celebrities, while the income inequality aspect is more likely to be emphasised by broadsheets). However, by looking at the effect of the tax day on unfairness perceptions, we can ascertain which signal is likely to outweigh on average.

2.3.3 The tax day widens the gap in support for redistribution between the rich and poor

We expect the tax day to widen the gap in support for redistribution between the rich and the poor. Specifically, we expect that less affluent individuals will respond to the tax day by increasing their support for redistribution, and that affluent individuals will respond by decreasing their support for redistribution.¹⁶

H3: *The tax day decreases (increases) support for redistribution amongst the (less) affluent.*

Several plausible mechanisms could explain why the tax day widens the gap in support for redistribution between income groups. First, by increasing the salience of inequality, the tax day might remind the (less) affluent that they would stand to (benefit) lose from redistribution. This expectation builds on experimental research which shows that exposure to visible markers of inequality makes the poor more likely to support redistribution (Sands and de Kadt, 2020) and the rich “double down” on their class interest (Côté et al., 2015; Nishi et al., 2015; Sands, 2017).¹⁷ Second, by focusing on the incomes of the super-rich, the tax day may reduce the perceived social status of the less affluent, and as a result increase their support for redistribution. This expectation builds on experimental research which shows that poor subjects who are induced to perceive greater social distance from the rich, express greater support for social welfare spending as a result (Condon and Wichowsky, 2020). Amongst the affluent, the tax day may also trigger status concerns and lead to a “keeping up with the Kardashians” reaction, where the welfare state and the associated tax burden are perceived as standing in the way of catching up with the super-rich. This expectation builds on experimental research which shows that affluent subjects become more economically conservative when exposed to information about others’ economic success (Thal, 2020). Finally, the tax day could widen the gap in support for redistribution by correcting individuals’ misperceptions about their relative position in the national income

¹⁶The expectation of heterogeneous effects builds on the income transparency literature, which has consistently found that individuals’ relative income position is a crucial factor moderating their response to income transparency. For example, Card et al. (2012) find that workers with below-median salaries report lower job satisfaction when incomes are made public, while those earning above the median remain unaffected. Similarly, Perez-Truglia (2020) finds that income transparency widens the subjective well-being gap between the rich and poor.

¹⁷In a field experiment, Sands (2017) finds that affluent subjects become less supportive of redistribution when randomly exposed to visibly poor person in their local neighbourhood. In a laboratory setting, Nishi et al. (2015) find that visible endowment inequality makes richer participants contribute less to their network. In a survey experiment, Côté et al. (2015) find that affluent participants become less generous (in a dictator game), when they are induced to believe that they lived in an unequal area.

distribution.¹⁸ This expectation builds on recent research which shows that individuals at the lower end of the income distribution tend to overestimate their relative income position, while those at the upper end of the income distribution tend to underestimate their relative income position (Cruces et al., 2013; Fehr et al., 2019; Hvidberg et al., 2020; Karadja et al., 2017).

2.3.4 The effect of the tax day is stronger amongst young people

We expect young people's redistributive preferences to be more responsive to the tax day compared to older age groups. This expectation builds on research in political socialisation, which suggests that individuals' political beliefs and preferences are malleable during adolescence and early adulthood, but relatively stable throughout later life (Bartels and Jackman, 2014; Dinas, 2013; Neundorf and Smets, 2017; Sears and Valentino, 1997). According to the "impressionable years" hypothesis, young people's political beliefs and preferences can be significantly shaped by socialising agents (such as parents or the media) as well as salient political events (such as recessions or elections) (Dinas, 2013; Neundorf and Smets, 2017). Later in life, however, such external stimuli leave individuals' political attitudes largely unaffected and attitudinal changes, if any, tend to be temporary (see Margalit 2013).

H4: *The effect of the tax day on support for redistribution is stronger amongst young people than amongst older age groups.*

While the idea of heightened sensitivity to attitudinal change during adolescence and early adulthood is widely accepted in the political socialisation literature (Neundorf and Smets, 2017), the mechanisms that might explain age differences in attitudinal stability in response to external influences are less well understood (Dinas, 2013). In the context of the Finnish tax day, a plausible mechanism behind the impressionable years hypothesis is that young people attach more weight to the income information revealed by the event. Two factors could explain such an age-specific information effect. First, the probability of exposure to a previous tax day may be lower for younger people, in which case the event simply reveals more "new" information about income inequality in Finland to young people. Second, even if the probability of exposure to a previous tax day is similar across age groups, young people may attach more weight to the information revealed by the tax day because they are less likely to interpret it through the lens of a consolidated political worldview (see Dinas 2013). While the impressionable years hypothesis makes no explicit prediction about

¹⁸The repeated nature of the tax day makes this mechanism less likely, as most citizens will not receive much new information about their relative income from being exposed to an additional tax day. An exception may be young people who are less likely to have experienced previous tax days (see Section 2.3.4 below).

the direction of the effect of the tax day on young people's support for redistribution, we expect it to be positive on average. This is because young people are disproportionately represented at the bottom of the income distribution.¹⁹

2.4 Empirical strategy

2.4.1 Methods

The unique institutional setting of Finland's tax day makes it possible to identify the causal effect of income transparency on individuals' attitudes in a before-and-after type research design. We take advantage of the fact that the tax day coincides with the implementation period of the ESS in Finland. Whether respondents took part in the ESS shortly before or after the tax day can be considered as-if-random, so we can estimate the causal effect of the tax day by comparing responses shortly before and after the event. This approach is sometimes referred to as *Unexpected Event during Survey Design*, and has been used to study the effect of events such as terrorist attacks (Finseraas and Listhaug, 2013; Legewie, 2013; Muñoz et al., 2020), election victories (Giani and Méon, 2019), leadership transitions (Mikulaschek et al., 2020), and football victories (Depetris-Chauvin et al., 2020). Valid identification relies on two key assumptions: temporal ignorability and excludability (Muñoz et al., 2020). Temporal ignorability means that the moment at which each respondent is interviewed during the fieldwork is independent from the timing of the tax day. Balance tests on pre-determined covariates (age, gender, education, etc.) suggest that this assumption is plausible within a 10-day window around the tax day (see Figure A2 in the appendix). Further away from the tax day, as-if random treatment assignment is less plausible given that respondents who are harder to reach are more likely to be interviewed later in the fieldwork period (see Figure A4 in the appendix). Using an even narrower window around the tax day in turn makes our estimates susceptible to bias from day-to-day variation in the number and types of respondents interviewed each day (see Figure 2.3 below).²⁰

Excludability means that the timing of the survey interview only affects the outcome of interest through the respondent's exposure to the tax day. Threats to identification can arise

¹⁹In the Finnish ESS data from 2002-18, the youngest age group (15-24 years) has a lower average household income than any other age group except for the oldest age group (75-100 years). The differences between age groups are statistically significant ($p < 0.001$).

²⁰Although we do not employ a regression discontinuity (RD) design, we note that a 10-day window minimises the mean-squared error (MSE) of the local polynomial RD point estimator (MSE-optimal bandwidth = 11.6).

from simultaneous events and from time trends in the outcome variable (Muñoz et al., 2020). While the excludability assumption cannot be directly tested, we present results from several placebo tests, which support our identification strategy. First, we show that the tax day has no significant effects on a placebo outcome (attitudes towards gays and lesbians). Second, we re-run our main analysis on ESS respondents from Sweden and find null effects. Third, we test for effects of a “fake” tax day prior to the actual tax day and find null effects (see Section E in the appendix).

We estimate the effect of the tax day using the following OLS model:

$$Y_{it} = \beta_1 Treatment_{it} + \beta_2 Days_{it} + \beta_3 (Treatment_{it} \times Days_{it}) + \gamma_t + \varepsilon_{it} \quad (2.1)$$

where Y_{it} refers to the outcome of interest (support for redistribution, unfairness perception, income comparison), $Treatment_{it}$ is a dummy equal to one on and after the tax day, and zero before the tax day, $Days_{it}$ is a running variable indicating the number of days before and after the tax day (with zero on the tax day itself), and γ_t refers to survey year fixed effects. The main coefficient of interest is β_1 , which captures the size of the discontinuity in the outcome on the tax day. The coefficient on the interaction term β_3 in turn indicates whether the treatment effect changes (weakens or strengthens) as time goes by after the tax day. Finally, β_2 captures linear time trends in the outcome variable prior to the tax day. We follow Muñoz et al. (2020) and use conventional standard errors, as they have a very similar setup with ESS data from a single country.²¹

In the baseline model, we restrict the sample to a 10-day window around the tax day in a given year, as covariate balance tests suggest that as-if-random treatment assignment is plausible within this window (see Figure A2). As a robustness check, we also present estimates for alternative bandwidths of 5 to 30 days around the tax day. In the baseline model, we furthermore exclude all respondents who were interviewed in the three days prior to the tax day. We do this because the media coverage of the tax day typically builds up for a few days before the event, so these respondents may have already been “treated” by the tax day. The build-up in media coverage is reflected in the number of tax day-related keyword hits that we observe just before the event (see Figure 2.2). As a robustness check, we also present results for alternative exclusion windows just before the tax day.

²¹For completeness we also report the main results with robust standard errors clustered at the level of the running variable (see Appendix B).

2.4.2 Data

We use data from all available rounds of the Finnish ESS (2002-2018).²² The ESS is a nationally representative survey that has been implemented in Finland every two years since 2002. The fieldwork period is typically from September to December, with a few interviews also conducted into the next year. Figure 2.3 shows that the tax day falls roughly into the middle of the fieldwork period, and that there is no obvious bunching of respondents before or after the event.²³ We interpret this as further evidence in support of the temporal ignorability assumption. Figure 2.3 also shows that survey enumerators conducted fewer interviews on Fridays, Saturdays and Sundays. Given that the tax day usually occurs on the first working day of November, we therefore record substantially fewer respondents in the 2-3 days just before the event.²⁴ These respondents may differ systematically from other respondents, for example because the ESS fieldwork guidelines require that unsuccessful interview attempts must be followed-up at the weekend.²⁵ While excluding three days prior to the tax day goes some way to address this concern, we also show that our results are robust to including day-of-the-week fixed effects in our regression models (Appendix C).

The fieldwork is implemented by Statistics Finland in collaboration with the Department of Social Research at the University of Turku. The data are collected through face-to-face computer assisted personal interviews (CAPI) in either Finnish or Swedish. The sample is selected by one-stage random sampling and is representative of all persons aged 15 and over who reside in private households.²⁶ Quota sampling and substitution of non-responding households or individuals are not permitted. The ESS aims for a response rate of at least 70% and the Finnish sample typically includes around 2000 respondents per survey round. The ESS fieldwork guidelines require at least four personal visits to each sample unit before it is abandoned as non-productive.²⁷ Figure A4 in the appendix plots the relationship between the number of attempted contacts with sampled units and the fieldwork day when the interview was completed. It shows that respondents who are harder to reach are more likely to be

²²The ESS data are available at: <https://www.europeansocialsurvey.org>

²³Figure A3 in the appendix provides a more fine-grained picture by zooming in on the 40-days around the tax day.

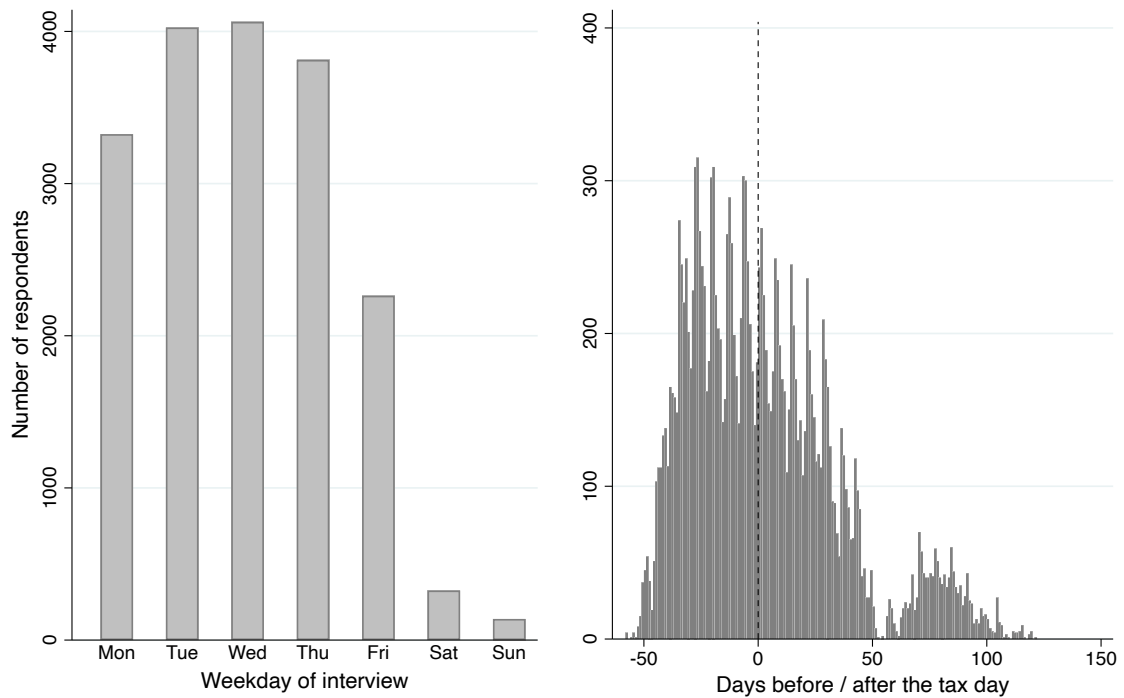
²⁴This is confirmed by a non-parametric density test (see Cattaneo, Jansson and Ma 2018), which rejects the null hypothesis that there is no discontinuity in the density of the running variable at the threshold ($t=1.9$; $p=0.06$).

²⁵Field Procedures in the European Social Survey Round 9: Guidelines for Enhancing Response Rates and Minimising Nonresponse Bias (p.10).

²⁶<https://www.europeansocialsurvey.org/about/country/finland/finnish/methods.html>

²⁷See Field Procedures in the European Social Survey Round 9: Guidelines for Enhancing Response Rates and Minimising Nonresponse Bias (p.10).

Fig. 2.3 Number of respondents by weekday and interview date (ESS 2002-18)



Data: ESS Finland 2002-18. *Note:* The left panel shows the number of survey respondents by weekday on which the interview was conducted. The right panel shows the number of respondents by interview date relative to the tax day. Exact dates for the tax days are found in Table A1. Figure A3 zooms in on the 40 days before and after the tax day.

interviewed later in the fieldwork period. This suggest that our strategy of focusing on a narrower 10-day window around the tax day is advisable to avoid potential biases related to reachability (Muñoz et al., 2020).

Table 2.1 provides summary statistics for the dependent variables used in the analysis. Our main dependent variable (support for redistribution) captures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from one (disagree strongly) to five (agree strongly). This measure of support for redistribution is quite general,²⁸ and may tap into respondents' attitudes towards the appropriate size of government. As a robustness check, we therefore use four alternative measures of support for redistribution, which capture, respectively, respondents' support for

²⁸For example, it does not distinguish between redistribution "from the rich" and "to the poor" (Cavallé and Trump, 2015).

unemployment benefits, their support for public childcare, their preference for economic equality, and their support for a social safety net (see Table A2 for details).

Table 2.1 Summary statistics

	Years	Obsv.	Mean	SD	Min	Max
Support for redistribution	2002-18	17766	3.92	0.99	1	5
Unfairness perception (of top 10% incomes)	2018	1681	5.70	1.53	1	9
Income comparison (with “others”)	2006	998	0.12	0.33	0	1

Data: ESS Finland 2002-18. *Note:* Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels. Unfairness perception captures how fair respondents think the incomes of the top 10% in Finland are, with high values reflecting higher unfairness perceptions. Income comparison identifies respondents who report being most likely to compare their income to others.

To our knowledge, available national surveys implemented in Finland during the relevant time period do not include standard issue salience questions, such as what respondents think is the most important problem facing the country (Moniz and Wlezien, 2020). We therefore use an 2018 ESS survey question on unfairness perceptions of top incomes as a proxy to capture the salience of income inequality at the individual level. The survey item prompts respondents to think about the 10% of employees working full-time in Finland who earn more than €6000 per month, and whether they consider these incomes unfairly low, fair, or unfairly high. Possible responses range from one to nine, with high values reflecting perceptions that top 10% incomes are unfairly high.

To measure income comparison, we use an item from the 2006 ESS, which asks respondents whose income they would be most likely to compare their own income to. The response options are (1) work colleagues, (2) family members, (3) friends, (4) others, (5) don’t compare, and (6) don’t know. Given that we are primarily interested in upward income comparisons (rather than comparisons with colleagues, family or friends), we create a binary variable that equals one for respondents who report being most likely to compare their income to “others”, and zero otherwise. While this measure is limited in the sense that we do not know who respondents think of as belonging into the “others” category, we can rule out colleagues, family, and friends.

Our measure of household income is based on respondents’ self-placement into national income deciles, which are pre-determined for each ESS round and calculated using income data from the Finnish tax registry. In the ESS 2002-6, EU-wide income categories were used instead of national income deciles. For these years, we impute national income deciles by

assigning each respondent an income that is drawn from a uniform random distribution of values between the lower and upper cut-off values of the EU-wide income bracket that they placed themselves in. For outcomes that are available for several survey years (e.g. support for redistribution), we have sufficient observations to disaggregate the analysis by income deciles. However, for outcomes that are only available for one survey year (e.g. unfairness perceptions), we have relatively small sample sizes (see Table 2.1), so we only distinguish between below- and above-median income earners.

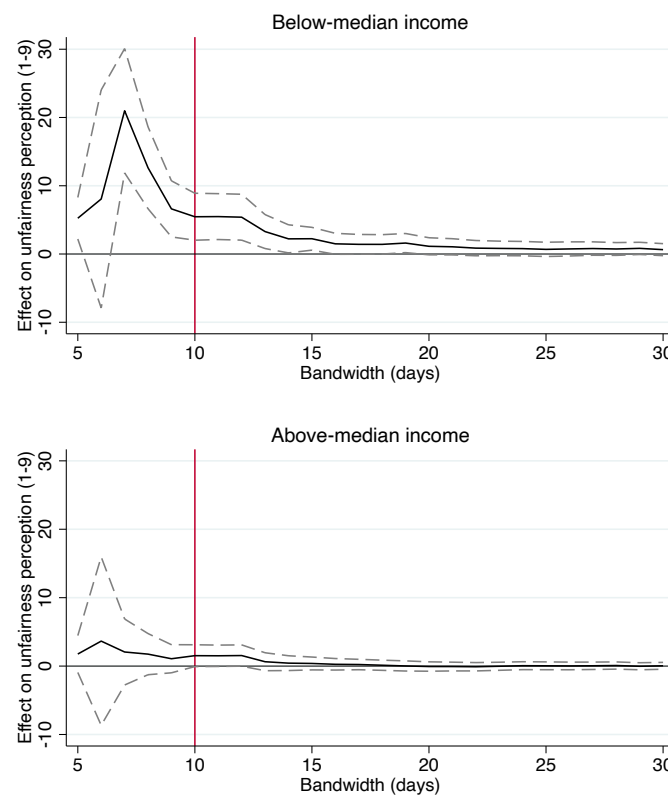
2.5 Results

2.5.1 The tax day increases unfairness perceptions

We find that the tax day increases perceptions that the incomes of the top 10% are unfairly high. Figure 2.4 plots the estimated effect of the tax day on unfairness perceptions separately for below- and above-median income earners and for different bandwidths of up to 30 days around the tax day. The red vertical line marks the default 10-day bandwidth. For below-median income earners, we observe a significant positive effect for all bandwidths up to 21 days. For above-median income earners, we only observe a significant positive effect for bandwidths between 10 and 13 days. Table A3 in the appendix presents the corresponding regression results from our baseline model with a 10-day bandwidth. In line with Hypothesis 1a, the results suggest that the effect of the tax day on unfairness perceptions is driven by the reactions of below-median income earners, for whom the effect is around 3.5 times larger than for above-median income earners.

For the whole sample (including both income groups), we observe a positive and statistically significant effect of 2.5 (see Table A5 in the appendix), which amounts to an increase in unfairness perceptions of more than 1.5 standard deviations - a relatively large effect size. The positive average effect on unfairness perceptions indicates that the tax day primarily increases the salience of inequality amongst the public, rather than signalling that “hard work pays off” (Hypothesis 1b) or that “the rich pay their fair share” (Hypothesis 1c). If one of these alternative signals had outweighed on average, we would have observed a negative or null effect respectively.

Fig. 2.4 Effect of the tax day on unfairness perception (varying bandwidths)



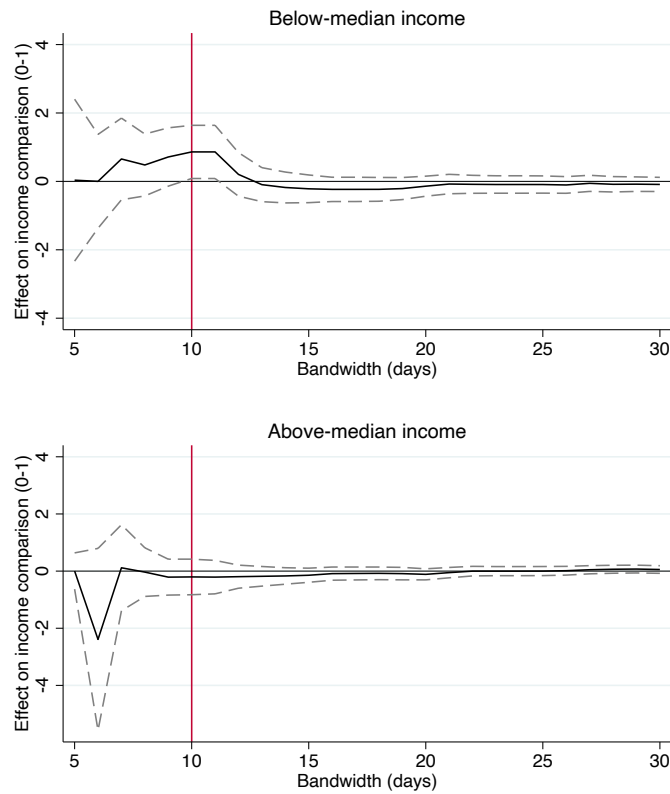
Data: ESS Finland 2018. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' unfairness perception for varying bandwidths (days) around the tax day. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. The results are presented separately for below-median income earners and above-median income earners. The red vertical line marks the default bandwidth of 10 days around the tax day.

2.5.2 The tax day increases income comparisons

We find that the tax day increases the likelihood that respondents compare their income to that of “others”, in line with Hypothesis 2. However, we only observe this effect for below-median income earners and not for above-median income earners. Figure 2.5 plots the estimated effect of the tax day on income comparison separately for below- and above-median income earners and for different bandwidths of up to 30 days around the tax day. For below-median income earners, we observe a significant positive effect of the tax day on income comparison for bandwidths between 8 and 12 days. For above-median income earners, the estimates remain indistinguishable from zero regardless of the bandwidth. Table A3 in the appendix presents the corresponding regression results from our baseline model

with a 10-day bandwidth. The results suggest that the tax day triggers income comparisons primarily amongst individuals at the lower end of the income distribution.

Fig. 2.5 Effect of the tax day on income comparison (varying bandwidths)



Data: ESS Finland 2006. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' income comparison for varying bandwidths (days) around the tax day. Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to "others", and zero otherwise. The results are presented separately for below-median income earners and above-median income earners. The red vertical line marks the default bandwidth of 10 days around the tax day.

Given that the outcome variable is binary in this case, the coefficients from the OLS model should be treated with caution and do not lend themselves to interpretation in terms of standard deviations. In Table A7 in the appendix, we present results from a binary logistic model. In the logistic model, the coefficient on the *Treatment* variable points in the same direction as in the OLS model, but it falls short of the conventional significance threshold ($p > 0.05$).

2.5.3 The tax day decreases support for redistribution only amongst top-earners

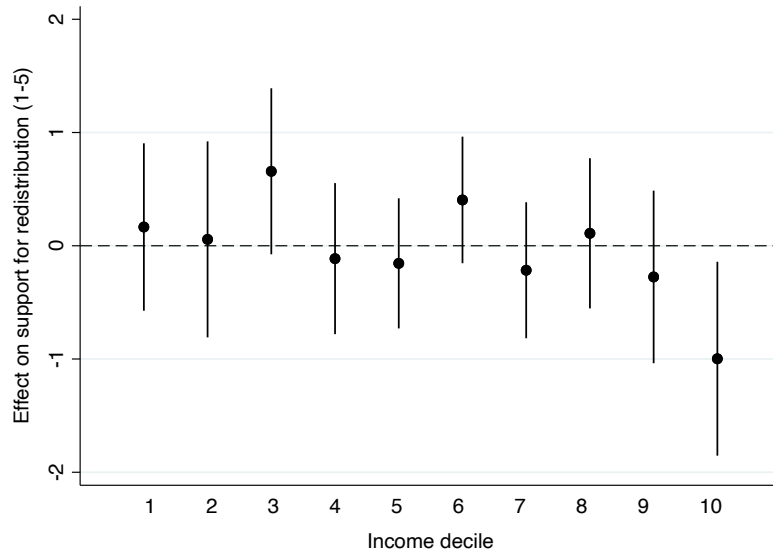
We find that the tax day leaves individuals' support for redistribution largely unaffected. Given that support for redistribution was measured in every ESS round since 2002, we can disaggregate the analysis by income deciles. Figure 2.6 plots the estimated effect of the tax day on support for redistribution for each income decile, using the default 10-day bandwidth. The effect of the tax day is indistinguishable from zero for all income groups except for the top income decile ($n = 314$), where it is negative ($\beta = -1$) and statistically significant ($p < 0.05$). The point estimate suggests that amongst the top income decile, the tax day decreases support for redistribution by around one standard deviation - which is a large effect size. For the whole sample (including all income groups), the estimated effect of the tax day on support for redistribution is statistically insignificant (see Table A5 in the appendix). The overall null effect is precisely estimated. We can rule out increases in overall support for redistribution that are larger than 0.2 standard deviations and decreases that are larger than -0.16 standard deviations - which are small effect sizes. Taken together, the results suggest that the tax day leaves support for redistribution largely unaffected, except amongst Finland's top-earners, where the event triggers a relatively strong negative response.

The result for the top income decile withstands several robustness checks. First, the result holds when we include day-of-the-week fixed effects (see Figure A8). Second, the result is relatively robust to alternative bandwidth choices. We find significant negative effects for all bandwidths between 7 and 13 days around the tax day (see Figure A9). Third, the result holds when using alternative exclusion windows from 1 to 5 days prior to the tax day (rather than the default 3 days) although the estimates are in some cases only significant at 90% (see Figure A10).

2.5.4 The tax day increases support for redistribution only amongst young people

Next, we disaggregate the analysis by age groups. Figure 2.7 plots the estimated effect of the tax day on support for redistribution for different age groups, using the default 10-day bandwidth specification. For most age groups, the estimates are indistinguishable from zero, which suggests that the tax day leaves their support for redistribution unaffected. A notable exception, however, are the youngest age group (15-24 years), where the tax day triggers

Fig. 2.6 Effect of the tax day on support for redistribution by income decile

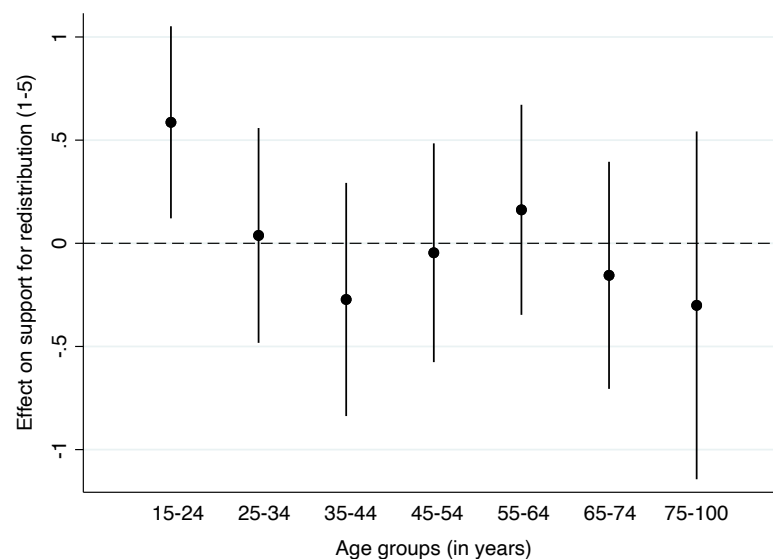


Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). Corresponding regression results are in Table A8.

an increase in support for redistribution of more than 0.5 standard deviations ($\beta = 0.58$; $p = 0.01$) - which is a medium-sized effect.

The result for the youngest age group withstands several robustness checks. First, the result holds when we include day-of-the-week fixed effects (see Figure A11). Second, the result is robust to alternative bandwidth choices. We find significant positive effects for all bandwidths of up to 15 days around the tax day (see Figure A12). Third, we consistently find positive effects for the youngest age group when using alternative exclusion windows from 1 to 5 days prior to the tax day. We note, however, that the estimates for exclusion windows of 4 and 5 days do not reach conventional significance (see Figure A13). Fourth, we find the same pattern (with noisier estimates) when controlling for household income (see Figure A14), which suggests that age matters independently of income. Finally, we run the analysis using smaller 4-year bins to determine age groups (see Figure A15). The estimates are noisier due to the smaller sample sizes, but they point in the same direction as the main results. The tax day triggers a positive and statistically significant ($\beta = 0.74$; $p = 0.01$) increase in support

Fig. 2.7 Effect of the tax day on support for redistribution by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 9-year bins. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). Corresponding regression results are in Table A10.

for redistribution amongst the youngest age group (15-19 years). The estimated effect for the 20-24 year-old's is also positive, but smaller and non-significant ($\beta = 0.28$; $p > 0.05$), which suggests that the reaction is concentrated amongst the very youngest respondents.

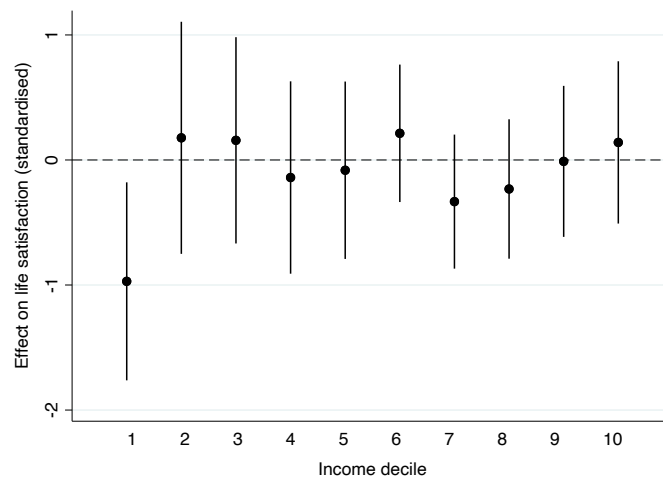
2.6 Mechanisms

2.6.1 Why does the tax day leave support for redistribution largely unaffected?

Weak treatment - Given that the tax day takes place every year, it is plausible that the “treatment” of the tax day is simply not strong enough to shift individuals' attitudes in a manner that is comparable to the effect of income transparency interventions in other contexts (Card et al., 2012; Perez-Truglia, 2020). As an external validity check, we therefore assess

whether the tax day affects individuals' subjective well-being. We look at subjective well-being given that this outcome has been previously studied in the very similar context of Norway, where citizens' tax records became easily accessible online in 2001 (Perez-Truglia, 2020). We measure subjective well-being using responses to the following question, which was included in all ESS rounds since 2002: "All things considered, how satisfied are you with your life as a whole nowadays?" Possible responses range from one (extremely dissatisfied) to 11 (extremely satisfied). Following Perez-Truglia (2020), we use the Probit-OLS method to assign values to each response option and then standardise the variable to a mean of 0 and a standard deviation of 1.²⁹ While self-reported measures of well-being have some limitations (e.g. due to social desirability bias), they have been shown to be significantly correlated with objective measures of well-being (Di Tella et al., 2003).

Fig. 2.8 Effect of the tax day on life satisfaction by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' life satisfaction by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Life satisfaction is measured with the following question: "All things considered, how satisfied are you with your life as a whole nowadays?" Response options range from 1 (extremely dissatisfied) to 11 (extremely satisfied). The values are adjusted using the Probit-OLS method and standardised to have a mean of 0 and a standard deviation of 1.

Based on previous findings from Norway (Perez-Truglia, 2020), we expect the tax day to increase the subjective well-being of the affluent and decrease the subjective well-being of

²⁹The Probit-OLS method assigns values to match the distribution of responses to a normal distribution (Ferrer-i Carbonell, 2005). The standardised Probit-OLS adjusted measure has 11 discrete values and runs from -3.2 to 1.8. See Figure A6 for a histogram of the re-scaled life satisfaction variable.

the less affluent. We find some evidence for this, although reactions to the tax day appear to be concentrated at the bottom of the income distribution. Figure 2.8 plots the estimated effect of the tax day on self-reported life satisfaction by income decile, using the default 10-day bandwidth specification. For most income deciles, the tax day leaves self-reported life satisfaction unaffected. However, for the bottom income decile, we observe a sizeable and statistically significant decrease in life satisfaction in response to the tax day (-0.9 SD; $p = 0.02$). In the appendix, we present evidence to suggest that a plausible mechanisms linking the tax day to decreased life satisfaction amongst the poorest is via its effect on perceived income status (see Figure A17). We interpret this as evidence that the tax day “treatment” is strong enough to shift attitudes in a manner that is similar to the effect of income transparency interventions studied in other contexts (Card et al., 2012; Perez-Truglia, 2020).³⁰

Information saturation - A related explanation for the overall null effect is that, because the tax day has taken place every year since 2000, most citizens’ do not obtain any new information from the event. If this is the case, we would expect to not observe null effects in early rounds of the ESS, where the tax day arguably still revealed more new information to Finnish citizens. Figure A16 plots the effect of the tax day on support for redistribution separately for each survey round between 2002 and 2018. To avoid small sample sizes, we only distinguish between below- and above-median income earners. We find no evidence that the tax day triggers stronger attitudinal reactions in early rounds of the ESS. While this evidence is only suggestive, we interpret it to mean that information saturation is unlikely to be the main reason why we observe an overall null effect of the tax day on support for redistribution.

Ceiling effects - Another plausible explanation for the overall null effect is that support for redistribution in Finland is quite high to begin with (Figure A5). To address concerns about ceiling effects, we turn to four alternative measures of support for redistribution, where responses are less clustered at the higher end of the scale (Figure A7). As for our main outcome, we find that the tax day leaves these alternative measures of support for redistribution largely unaffected (Table A12). This suggests that our main null result is unlikely to be driven by ceiling effects.

Partisanship - It is also possible that the overall effect of the tax day on support for redistribution is limited because many citizens identify with a specific political party and follow the cues of their preferred political party on the issue of redistribution (Cavaille, 2020).

³⁰Note that our estimates are nor directly comparable, as these studies do not disaggregate their results by income decile. In Norway, Perez-Truglia (2020) finds that income transparency increased the life satisfaction–income gradient by 21%. In California, Card et al. (2012) find that income transparency decreased self-reported job satisfaction by 0.22 standard deviations amongst respondents in the lowest income quartile.

If this is the case, we would expect to observe null effects amongst citizens with partisan attachments, but to observe changes in support for redistribution amongst citizens with *no* partisan attachments. While the majority of our sample (56%) do indeed identify with a specific political party, we find no evidence that their response to the tax day is more muted than the response of non-partisans (see Figure A18). While this evidence does not rule out that partisanship might explain the muted overall response to the tax day, it casts some doubts on this explanation.

Political ideology - Finally, it is possible that the overall null effect of the tax day on support for redistribution hides divergent reactions amongst left- and right-wing respondents. This idea is supported by recent research (Fenton, 2020; Karadja et al., 2017), which suggests that political ideology can be an important moderating factor determining whether and how much citizens' adjust their support for redistribution when exposed to information about inequality. Figure A19 in the appendix shows the estimated effect of the tax day on support for redistribution separately for left- and right-wing respondents.³¹ There is no evidence that the overall null result is driven by divergent reactions amongst left- and right-wing respondents.

2.6.2 Why does the tax day decrease support for redistribution amongst the top income decile?

There are at least four plausible mechanisms that could explain why respondents in the top income decile respond to the tax day by decreasing their support for redistribution. While the available data do not allow us to confirm or rule out any of these complementary mechanisms, we do find some evidence in support of one mechanism (motivated reasoning) and little evidence in support of the others. We discuss each potential mechanism in turn.

Correcting misperceptions - The first potential mechanism is that the tax day corrects misperceptions amongst the top 10% about their relative position in the income distribution. We might expect a stronger corrective information effect amongst the top 10% because the very rich tend to underestimate their relative income status more than any other income group (Hvidberg et al., 2020).³² However, we think that this mechanism is unlikely to explain our results for the following reasons. The tax day focuses on the incomes of the super-rich who earn more than €100,000 a year, so perceptions of relative income status amongst the top

³¹The correlation between ideological self-placement (on a 10-point left-right scale) and support for redistribution (on a 5-point scale) is negative and statistically significant ($r = -0.28$; $p < 0.01$).

³²Hvidberg et al. (2020) use survey and administrative data from Denmark to show that people with incomes above the 95th percentile overestimate the average income in the 95th percentile by 50%.

10% should become even more biased downwards, if anything. Furthermore, we find no evidence that respondents in the top income decile adjust their perceived relative income status in response to the tax day. Unfortunately, we do not have data on perceived income rank comparable to Hvidberg et al. (2020), so we cannot directly quantify misperceptions about relative income. However, since 2002, the ESS includes an item on perceived income adequacy, which we use as a proxy to measure perceived income status. Figure A17 in the appendix shows that the tax day has no significant effect on perceived income adequacy amongst the top income decile. While this evidence is only suggestive, it speaks against the idea that the top 10% reduce their support for redistribution in response to the tax day because the event corrects their (mis-)perceptions about their relative income status.

Salience of tax burden - A second possible mechanism is that the tax day suppresses support for redistribution amongst the top income decile because it reminds these individuals of their relatively high tax burden. This is plausible because the tax day reveals the total amount of taxes paid by Finland's top-earners and the newspapers' search engines (*Verokone*) usually report their tax rate as well.³³ In this scenario we would expect the tax day to decrease the proportion of respondents in the top income decile who agree with the statement that "higher earners should pay a higher share of earnings in tax". We find no evidence that this is the case (see Figure A20). However, the null result may be driven by the small number of observations, as the relevant item was only included in one survey round.

Status concerns - A third plausible mechanism is that the tax day suppresses support for redistribution amongst the top income decile because it triggers a "keeping up with the Kardashians" reaction. Such a reaction is plausible because the tax day facilitates upward income comparisons with the super-rich who earn more than €100,000 a year.³⁴ If status concerns are the mechanism linking the tax day to reduced support for redistribution amongst the top income decile, we would expect the event to increase the extent to which the top 10% report valuing money and other status goods. However, we find no evidence that the tax day increases the proportion of respondents in the top income decile who agree with the statement that it is "important to be rich, have money and expensive things" (see Figure A21). In this case, the relevant item was included in all ESS rounds, so the null effect is unlikely due to small sample size.

Motivated reasoning - A fourth plausible mechanism is that the tax day suppresses demand for redistribution amongst the top income decile because the event triggers a process

³³See e.g. <https://www.iltalehti.fi/verokone>. Finland's top-earners typically pay between 30-50% taxes on earned and capital income.

³⁴In line with this expectation, experimental research from the US shows that exposing affluent subjects to other people's success causes them to hold more economically conservative views (Thal, 2020)

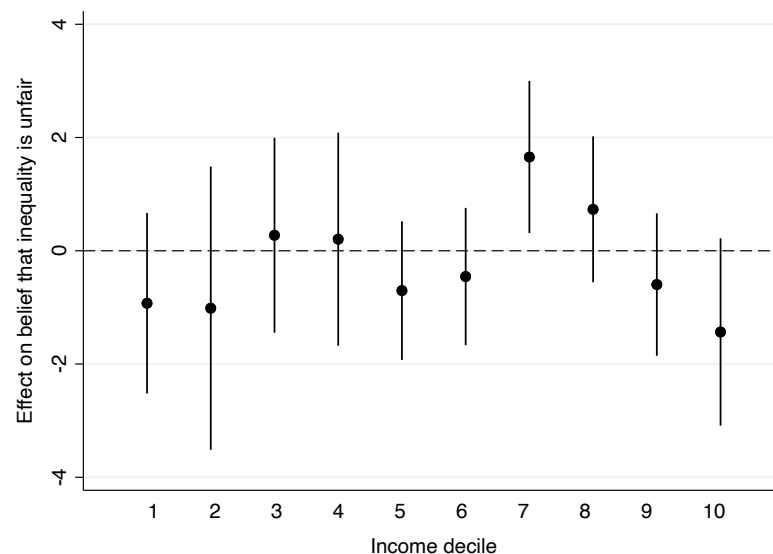
of motivated reasoning. Given that the tax day focuses on the incomes of Finland's top-earners, individuals in the top income decile may feel unfairly targeted or singled-out by the tax day and may therefore (re-)affirm their beliefs that income inequality in Finland is justified, for example because high incomes are rewards for effort or talents.³⁵ This explanation finds support in recent sociological research from Finland (Kantola, 2020; Kantola and Kuusela, 2019), which shows that the nation's top-earners tend to construct self-identities based on hard work in order to justify their wealth in the face of strong egalitarian norms. We find some suggestive evidence for the motivated reasoning mechanism. Figure 2.9 shows that, amongst the top income decile, the tax day decreases support for the statement that "for a fair society, differences in standard of living should be small." The estimate for the top income decile is negative and statistically significant ($\beta = -1.43$; $p = 0.08$), and amounts to a reduction of 1.5 standard deviations. The relevant item is only included in two ESS rounds, so the small sample size in the top income decile ($n = 83$) might explain the relatively large confidence interval. However, despite the noisy estimates, Figure 2.9 shows a rightward shift in fairness beliefs in response to the tax day amongst individuals at the top of the income distribution. Reassuringly, we find a very similar pattern when using an alternative measure of respondents' beliefs that inequality is justified or fair. The relevant item was included in two ESS rounds and asks respondents to what extent they disagree that "large differences in income are acceptable to reward talents and efforts", with higher values on the 5-point scale reflecting higher levels of disagreement. Again, although the estimates are noisy, they indicate that the tax day boosts beliefs amongst top-earners that inequality can be justified to reward talents or effort (see Figure A22).

2.6.3 Why does the tax day increase support for redistribution amongst young people?

Correcting misperceptions - A possible mechanism linking the tax day to increased support for redistribution amongst the youngest age group is through its effect on perceptions of relative income status. The information effect of the tax day might be stronger for the youngest age group, either because they are less likely to have been exposed to previous tax days, or because they are less likely to interpret the information through the lens of

³⁵In line with this mechanism, Suhay et al. 2020 find that affluent Americans are more likely than other income groups to attribute economic success to intelligence and hard work. They also find that "individual-blaming" attributions for economic success are more strongly predictive of economic conservatism amongst the affluent than amongst other income groups.

Fig. 2.9 Effect of the tax day on belief that inequality is unfair by income decile



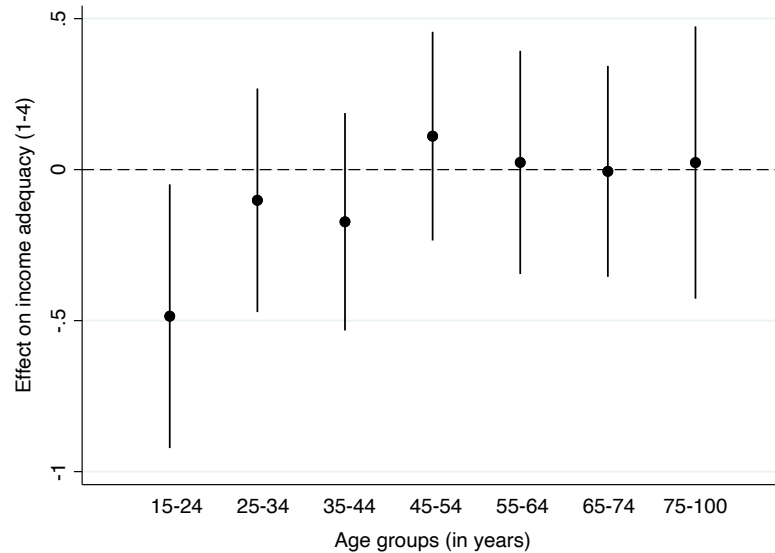
Data: ESS Finland 2008 & 2016. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' belief that inequality is unfair by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Belief that inequality is unfair measures respondents' support for the statement that "for a fair society, differences in standard of living should be small." Response options range from 1 (disagree strongly) to 8 (agree strongly) so that higher values reflect more left-wing beliefs.

a consolidated political worldview (see Dinas 2013).³⁶ Either way, we would expect the youngest age group to adjust their perceptions of relative income status more than older age groups. In this case, a leftward shift in redistributive preferences is plausible because young people are disproportionately represented at the lower end of the income distribution, where individuals tend to overestimate their relative income status (Hvidberg et al., 2020). We find some evidence in support of this explanation. As earlier, we use perceived income adequacy as a proxy for perceived income status. In line with our expectations, we find that the tax day decreases perceived income adequacy amongst those in the youngest age group, and only here (see Figure 2.10). The negative effect is sizeable and statistically significant (-0.7 SD; $p = 0.02$). While this evidence is only suggestive, it speaks for the idea that the tax day leads

³⁶15-24 year-olds are also least likely to have experience paying taxes. Only 26% of 15-24 year-olds are in paid work, compared with 73% of 25-65 year-olds.

to increased support for redistribution amongst the youngest age group because it corrects (mis-)perceptions about their relative income status.

Fig. 2.10 Effect of the tax day on perceived income adequacy by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perceived income adequacy by age group. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Perceived income adequacy captures how respondents feel about their household's income nowadays, with response options ranging from 1 (finding it very difficult) to 4 (living comfortably).

Media diets - An alternative explanation for the stronger reaction amongst the youngest age group is that young people systematically expose themselves to different media coverage of the tax day than older age groups (e.g. coverage that is more critical of income inequality). Unfortunately, the ESS does not ask respondents which newspapers they read or television channels they watch. However, we can measure *how much* respondents are exposed to news about politics and current affairs. We use an ESS item which asks respondents how much time they spend on a typical day watching, reading or listening to news about politics and current affairs (in minutes),³⁷ and divide the sample into respondents with high media exposure (>60 minutes per day) and low media exposure (<60 minutes per day).³⁸ We

³⁷This item was included in only two ESS rounds (2016 & 2018).

³⁸60 minutes is the median and mean response.

find that young people are significantly less likely to be exposed to news about politics and current affairs compared to older age groups. For example, only 31% of 15-24 year-old's have high media exposure compared with 73% of 75-100 year-old's.³⁹ This suggests that young people do indeed have a different media diet than older age groups. However, we find no evidence that the tax day affects support for redistribution differently depending on whether respondents have high- or low media exposure (see Figure A23). While this evidence is only suggestive, it casts some doubts on the idea that systematically different media diets explain why young people react more strongly to the tax day.

2.7 Discussion

Every year, on the first working day of November, Finland's tax authorities release the income information of everyone who earns more than €100,000 a year to the public. We propose that the so-called tax day increases the salience of income inequality in the public debate and expect it to widen the gap in support for redistribution between the rich and the poor. We use media data to show that the tax day coincides with a significant spike in media coverage related to income inequality. Using nationally representative survey data and a before-and-after type research design, we also show that the tax day increases income comparisons and perceptions that the incomes of the top 10% are unfairly high. However, despite these initial effects, we find that the tax day leaves public support for redistribution largely unaffected. We can rule out increases in support for redistribution that are larger than 0.2 standard deviations and decreases that are larger than -0.16 standard deviations. We explore possible explanations for the public's muted response to the tax day, and present evidence that the overall null effect on support for redistribution is unlikely due to the repeated nature of the tax day, ceiling effects, political partisanship, or divergent reactions amongst left- and right-wing respondents. Importantly, we show that the overall null effect hides substantial heterogeneity between income and age groups. We find that the tax day suppresses support for redistribution amongst individuals in the top income decile, and we present evidence that this effect likely operates via changes in top-earners' fairness beliefs. Furthermore, we find that the tax day increases support for redistribution amongst the youngest age group (15-24 years), and we provide evidence that this effect likely operates through changes in young people's perceived relative income status.

Regarding the external validity of our findings, we note that Finland is one of the most equal societies in Europe (OECD, 2011) and support for redistribution is relatively high in

³⁹The differences between the youngest age group and all older age groups are statistically significant.

comparison to other countries in Europe.⁴⁰ Although we present evidence that our results are unlikely due to ceiling effects, it is possible that the impact of income transparency is more pronounced in other contexts, where baseline support for redistribution is lower and income differences are larger. Furthermore, although several Nordic countries have adopted income transparency policies (see e.g. Bø et al. 2015), Finland's tax day is unique in that it creates an annual media spectacle focused on the nation's top earners (Barry, 2018). In Norway, for example, the availability of tailored search apps that were integrated into social media platforms such as *Facebook* meant that Norwegians primarily used income transparency to compare their own income with that of their neighbours and friends, rather than the nation's top-earners (Perez-Truglia, 2020). While it is difficult to rule out that the muted response to Finland's tax day is due to the specific nature of the event (rather than the nature of redistributive preferences), we provide evidence that the tax day has similar effects on citizens' subjective well-being as a comparable income transparency intervention in Norway (Perez-Truglia, 2020). This reassures us that the muted effect of income transparency on support for redistribution in Finland is not solely determined by the specific institutional context of the tax day.

A limitation of our research design is that we primarily focus on respondents who were interviewed within 10 days of the tax day. We do this because the assumption of as-if random exposure to the tax day is most plausible in such a narrow window. This approach comes at a cost in that we can only assess the short-term effects of the tax day. However, we note that this limitation is not unique to our study and pertains to most previous experimental studies that manipulate subjects' support for redistribution by exposing them to information about inequality (Condon and Wichowsky, 2020; Cruces et al., 2013; Karadja et al., 2017; Sands, 2017; Sands and de Kadt, 2020).⁴¹ Another limitation of our research design is that we only have data from 2002 onward, which is two years after the tax authorities first started releasing the income information of Finland's top-earners to the media. We disaggregate our analysis by survey round and find no evidence that the effect of the tax day on support for redistribution was more pronounced in early rounds. However, we cannot rule out entirely that the tax day had a significant effect on citizens' redistributive preferences when it first took place in 2000, and that the null effects we observe from 2002 onward are due to information saturation.

Overall, our findings suggest that income transparency can trigger greater concern about income inequality amongst ordinary citizens, but that it may only lead to marginal changes

⁴⁰See footnote 8

⁴¹An exception are Kuziemko et al. (2015), who re-survey respondents after one month and find that 58% of the initial effect size remains (p.1493).

in public support for redistribution. This interpretation corresponds well with a seminal study by Kuziemko et al. (2015), who show that randomly exposing survey respondents to information on income inequality has large effects on their views about inequality, but only slightly moves their support for redistributive policies. Our results contrast, however, with several recent survey- and field experiments (Condon and Wichowsky, 2020; Cruces et al., 2013; Sands, 2017; Sands and de Kadt, 2020), which find that subjects' support for redistribution can be shifted by exposing them to information about (or visible markers of) inequality. We contribute to this experimental research, by studying, for the first time, the effect of a real-world policy on support for redistribution in the context of a large-scale quasi-experiment. While the debate on the "malleability" of redistributive preferences is far from settled, our results indicate that triggering significant shifts in support for redistribution may be more difficult to achieve outside the controlled experimental setting.

An important implication of our findings is that lack of cross-class exposure may not be the main reason why public support for redistribution has failed to "keep up" with rising inequality in Western democracies. Scholars and political commentators, especially in the US context, frequently point to limited cross-class exposure due to residential or educational segregation as an explanation for the disconnect between rising inequality and lack of support for redistribution (e.g. Condon and Wichowsky 2020; Minkoff and Lyons 2019; Reardon and Bischoff 2011). In Finland, income transparency is also frequently justified as a mechanism to encourage cross-class comparisons and resist the trend towards growing inequality (Barry, 2018; Yläjärvi, 2020). We present evidence that income transparency does indeed trigger cross-class comparisons, but that it only marginally affects public support for redistribution. This casts some doubts on the idea that increasing cross-class exposure is all it takes for redistributive demand to catch up with rising inequality. Of course, income transparency is just one of many policy interventions that might increase cross-class exposure. For example, residential integration programmes like the *Moving to Opportunity* initiative (see Chetty et al. 2016, 2014) are likely to expose participants to visible markers of inequality. Exposure to inequality in local neighbourhoods may in turn have much more far-reaching consequences for redistributive demand than the relatively abstract exposure to the super-rich triggered by Finland's tax day (see Sands and de Kadt 2020). Exploring the attitudinal effects of other policies that might increase citizens' exposure to inequality is a promising avenue for further research.

A further implication of our findings is that income transparency is unlikely to trigger a "populist backlash" as feared by opponents of disclosure policies (Mas, 2017). In California, Mas (2017) shows that pay disclosure in the public sector reduced compensation of city managers by around 7%. He attributes this effect to a "populist" aversion to high salaries

amongst the public, but he stops short of examining whether pay disclosure shifted citizens' redistributive preferences. In Finland, we find no evidence that income transparency triggers a left- or right-ward shift in redistributive preferences, which casts some doubts on the idea that disclosure policies will affect citizens' political opinions more broadly. Further research could explore whether income transparency impacts other possible measures of "populist" sentiment amongst citizens, such as their support for radical right parties.

It is beyond the scope of this paper to explore in detail *why* citizens' redistributive preferences are relatively inelastic to increasing exposure to inequality. However, we think it is plausible that citizens' redistributive preferences are rooted in stable political worldviews that are difficult to alter once they are crystallized during the "impressionable years" of adolescence and early adulthood ([Neundorff and Smets, 2017](#)). Our finding that the youngest age group (15-24 years) are more responsive to the tax day than older age groups supports this view. Our data do not allow us to establish precisely why young people attach more weight to the income information revealed by the tax day, but one plausible channel is that individuals in this age group are less likely than older age groups to interpret the information through the lens of a consolidated political worldview (see [Dinas 2013](#)). Further research is needed to unpack the precise mechanisms behind young people's heightened sensitivity to attitudinal change.

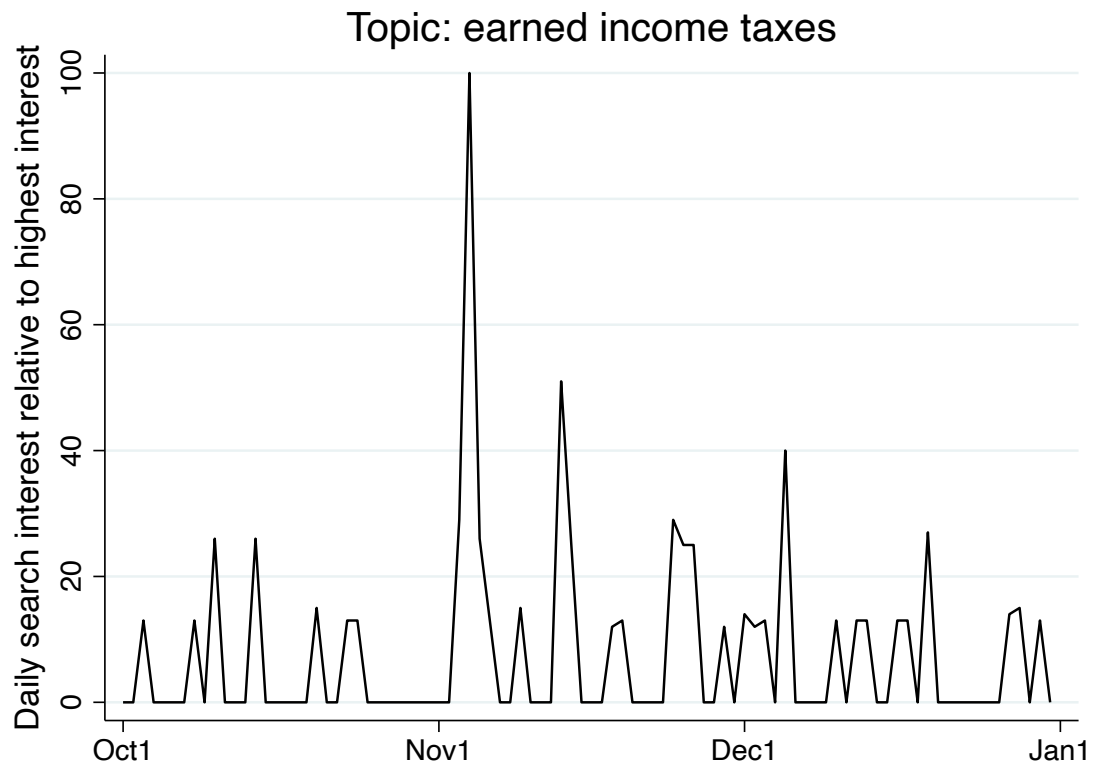
2.8 Appendix

A Supplementary descriptive material

Table A1 Finland's tax days (2000-2020)

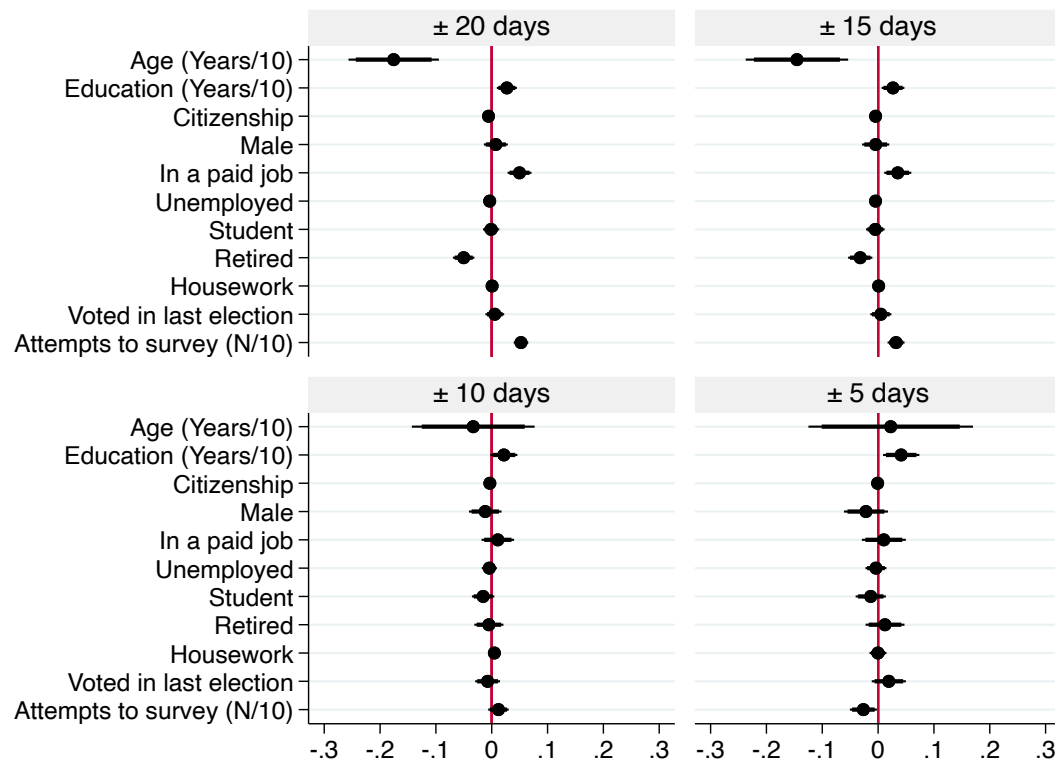
Year	Date	Day of the week
2000	Nov 1	Wednesday
2001	Nov 1	Thursday
2002	Nov 1	Friday
2003	Nov 3	Monday
2004	Nov 1	Monday
2005	Nov 1	Tuesday
2006	Nov 1	Wednesday
2007	Nov 1	Thursday
2008	Nov 3	Monday
2009	Nov 2	Monday
2010	Nov 1	Monday
2011	Nov 1	Tuesday
2012	Nov 1	Thursday
2013	Nov 1	Friday
2014	Nov 3	Monday
2015	Nov 2	Monday
2016	Nov 1	Tuesday
2017	Nov 1	Wednesday
2018	Nov 1	Thursday
2019	Nov 4	Monday
2020	Nov 3	Tuesday

Fig. A1 Google search queries for the “earned income taxes” topic (Oct–Dec 2019)



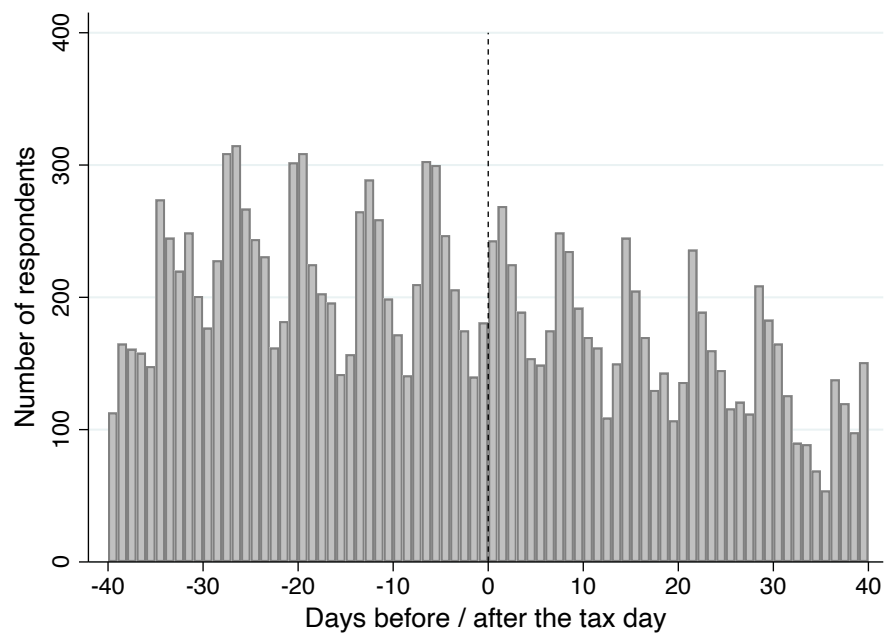
Data: Google Trends. *Note:* Numbers represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means that there was not enough data for this term. In 2019, the tax day was on Monday, November 4th.

Fig. A2 Balance tests on covariates (ESS 2002-18)



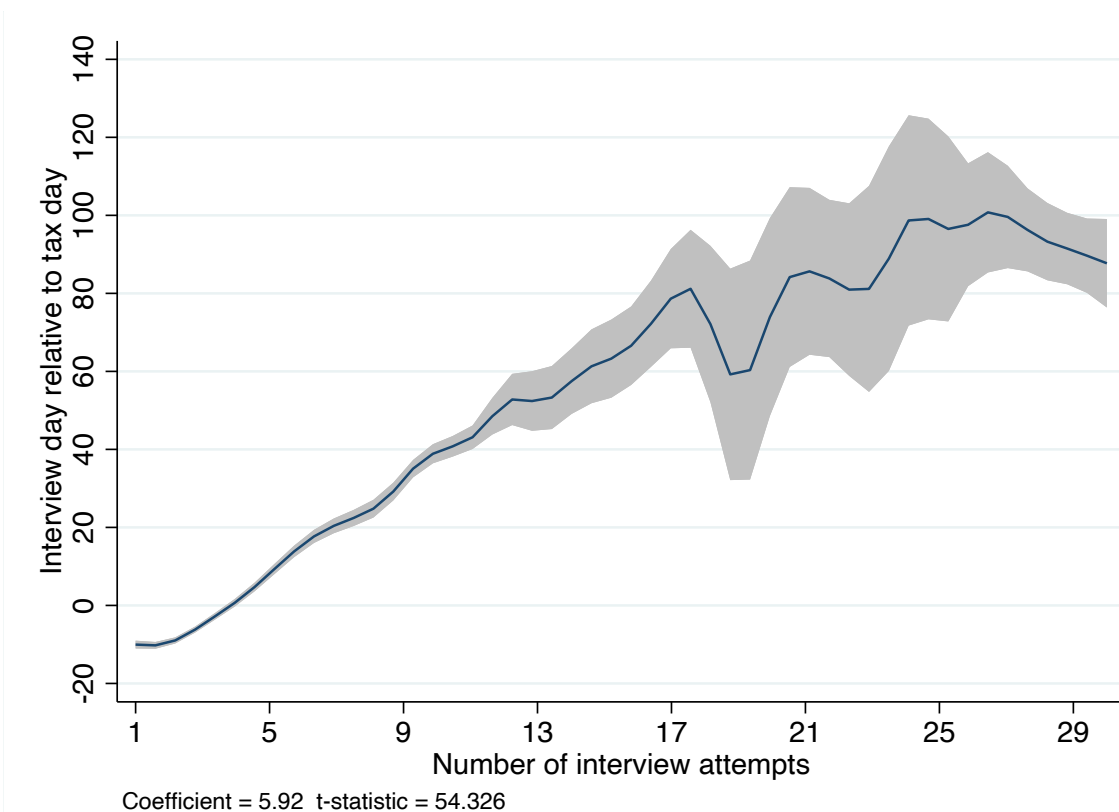
Data: ESS Finland 2002-18 for all covariates except “attempts to survey”, which is from ESS rounds 2008-2018. *Note:* Entries report the difference in the mean of the covariates between the treatment and control groups for various bandwidths (days) around the tax day. Thick and thin lines are 90% and 95% confidence intervals, respectively.

Fig. A3 Number of respondents by interview date in 40-day window around the tax day (ESS 2002-18)



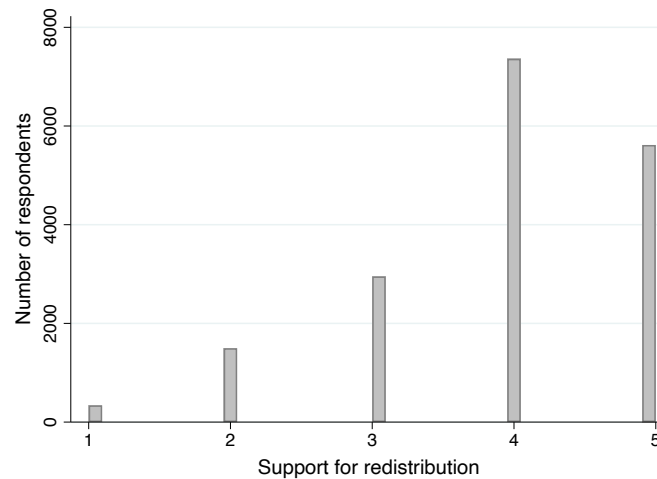
Data: ESS Finland 2002-18. *Note:* Exact dates for the tax days are in Table A1.

Fig. A4 Number of attempts to survey by interview day relative to tax day (ESS Paradata 2008-18)



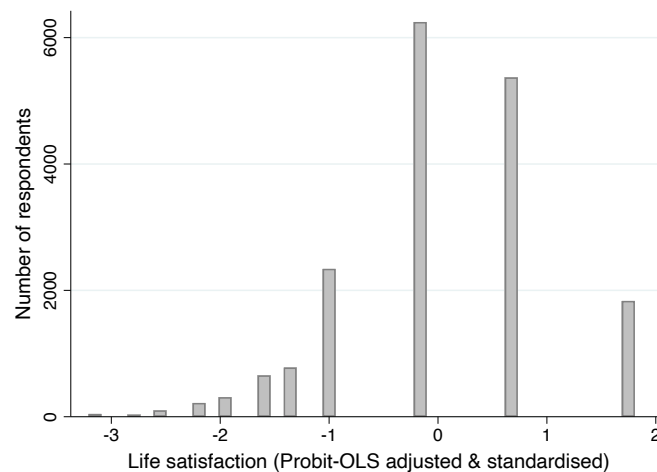
Data: ESS Paradata 2008-18 for Finland. Earlier rounds are not included because the coding of interview outcomes in the ESS Paradata changed from 2006 to 2008. *Note:* The figure plots the number of attempts to survey before interview completion by fieldwork day when the interview was completed (relative to the tax day). We use kernel-weighted local polynomial smoothing with 95% confidence intervals, as [Muñoz et al. \(2020\)](#). The coefficient and t-statistic are from an OLS model regressing fieldwork day when interview was completed on the number of attempts to survey.

Fig. A5 Histogram of support for redistribution



Data: ESS Finland 2002-18. *Note:* Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Fig. A6 Histogram of life satisfaction



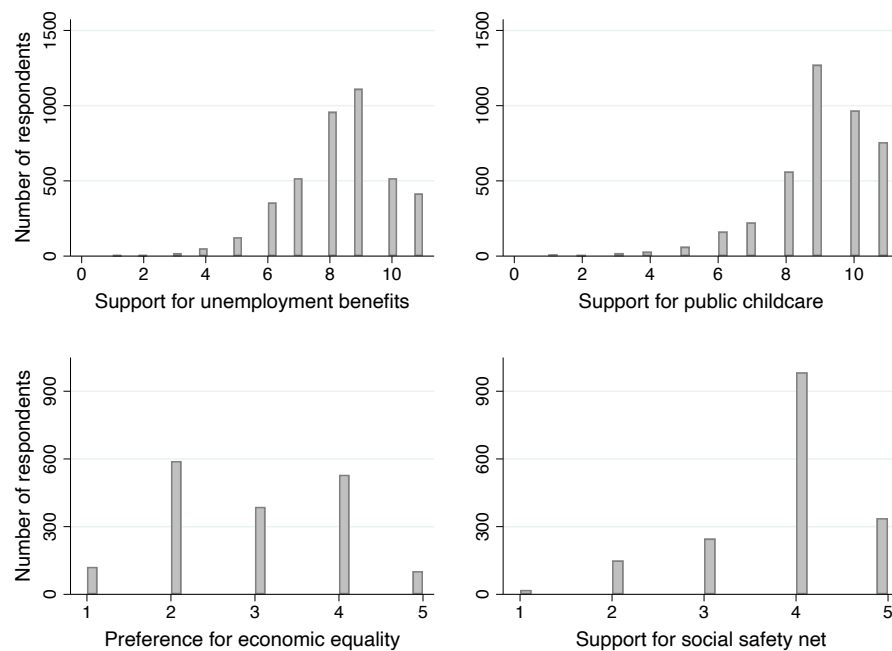
Data: ESS Finland 2002-18. *Note:* Life satisfaction is measured with the following question: “All things considered, how satisfied are you with your life as a whole nowadays?” Response options range from 1 (extremely dissatisfied) to 11 (extremely satisfied). The values are adjusted using the Probit-OLS method and standardised to have a mean of 0 and a standard deviation of 1.

Table A2 Summary statistics for alternative measures of support for redistribution

	Years	Obsv.	Mean	SD	Min	Max
Support for unemployment benefits	2008 & 2016	4092	8.34	1.68	1	11
Support for public childcare	2008 & 2016	4074	9.09	1.59	1	11
Preference for economic equality	2018	1734	2.94	1.08	1	5
Support for social safety net	2018	1739	3.85	0.87	1	5

Data: ESS Finland 2008, 2016, 2018. *Note:* Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

Fig. A7 Histograms of alternative measures of support for redistribution



Data: ESS Finland 2008, 2016, 2018. *Note:* Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

B Regression tables

Table A3 Effect of the tax day amongst below- and above-median income earners

	(1) Unfairness perception	(2) Income comparison	(3) Support for redistribution
Below-median income			
Treatment	5.461*** (1.732)	0.862** (0.389)	0.168 (0.156)
Days	-0.622*** (0.203)	-0.155*** (0.053)	-0.017 (0.021)
Treatment × Days	0.657*** (0.219)	0.175*** (0.055)	0.006 (0.023)
Survey-year FE	-	-	✓
Observations	94	67	1,606
R-squared	0.119	0.155	0.007
Survey years	2018	2006	2002-18
Above-median income			
Treatment	1.524* (0.806)	-0.204 (0.315)	-0.076 (0.149)
Days	-0.133 (0.093)	0.033 (0.040)	0.024 (0.020)
Treatment × Days	0.049 (0.108)	-0.038 (0.042)	-0.034 (0.022)
Survey-year FE	-	-	✓
Observations	153	153	2,051
R-squared	0.030	0.006	0.005
Survey years	2018	2006	2002-18

Note: Estimates are from OLS regressions with standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to “others”, and zero otherwise. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A4 Effect of the tax day amongst below- and above-median income earners (clustered standard errors)

	(1) Unfairness perception	(2) Income comparison	(3) Support for redistribution
Below-median income			
Treatment	5.461* (3.070)	0.689** (0.255)	0.140 (0.095)
Days	-0.622* (0.344)	-0.132*** (0.034)	-0.012 (0.013)
Treatment \times Days	0.657* (0.349)	0.150*** (0.037)	0.001 (0.017)
Survey-year FE	-	-	✓
Observations	94	67	1,606
R-squared	0.119	0.155	0.007
Survey years	2018	2006	2002-18
Above-median income			
Treatment	1.524*** (0.312)	-0.145 (0.269)	-0.055 (0.128)
Days	-0.133*** (0.042)	0.026 (0.033)	0.020 (0.017)
Treatment \times Days	0.049 (0.045)	-0.028 (0.033)	-0.031 (0.019)
Survey-year FE	-	-	✓
Observations	153	153	2,051
R-squared	0.030	0.006	0.005
Survey years	2018	2006	2002-18

Note: Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to “others”, and zero otherwise. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A5 Effect of the tax day (whole sample)

	(1) Unfairness perception	(2) Income comparison	(3) Support for redistribution
Treatment	2.538*** (0.793)	0.004 (0.235)	0.043 (0.106)
Days	-0.268*** (0.092)	-0.005 (0.031)	0.004 (0.014)
Treatment \times Days	0.240** (0.104)	0.009 (0.032)	-0.015 (0.016)
Survey-year FE	-	-	✓
Observations	247	231	3,789
R-squared	0.043	0.001	0.003
Survey years	2018	2006	2002-18

Note: Estimates are from OLS regressions with standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to “others”, and zero otherwise. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A6 Effect of the tax day (whole sample, clustered standard errors)

	(1) Unfairness perception	(2) Income comparison	(3) Support for redistribution
Treatment	2.538*** (0.767)	0.004 (0.250)	0.043 (0.092)
Days	-0.268*** (0.085)	-0.005 (0.031)	0.004 (0.011)
Treatment \times Days	0.240** (0.090)	0.009 (0.032)	-0.015 (0.013)
Survey-year FE	-	-	✓
Observations	247	231	3,789
R-squared	0.043	0.001	0.003
Survey years	2018	2006	2002-18

Note: Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Unfairness perception ranges from 1 to 9, with high values reflecting perceptions that top 10% incomes are unfairly high. Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to “others”, and zero otherwise. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A7 Effect of the tax day on income comparison (Logit)

	(1) Below-median income	(2) Above-median income
Treatment	9.702 (6.819)	-1.587 (2.318)
Days	-1.511* (0.852)	0.256 (0.307)
Treatment \times Days	1.692* (0.864)	-0.293 (0.319)
Observations	66	154
Survey years	2006	2006

Note: Estimates are from binary logistic regressions with standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the log-odds that respondents report being most likely to compare their income to “others”. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Model 1 shows estimates for the less affluent sub-sample (below-median income) and Model 2 shows estimates for the affluent sub-sample (above-median income). Income comparison is a binary variable equal to one for respondents who report being most likely to compare their income to “others”, and zero otherwise.

Table A8 Effect of the tax day on support for redistribution by income decile

Income decile	1 st	2 nd	3 rd	4 th	5 th	6 th	7 th	8 th	9 th	10 th
Treatment	0.165 (0.376)	0.056 (0.440)	0.657* (0.373)	-0.114 (0.339)	-0.155 (0.292)	0.405 (0.284)	-0.216 (0.305)	0.110 (0.337)	-0.275 (0.387)	-0.998** (0.435)
Days	-0.035 (0.049)	0.049 (0.060)	-0.085 (0.052)	0.021 (0.045)	0.016 (0.040)	-0.049 (0.038)	0.037 (0.042)	-0.022 (0.044)	0.071 (0.051)	0.157*** (0.057)
Treatment × Days	0.042 (0.055)	-0.095 (0.066)	0.050 (0.057)	-0.013 (0.050)	-0.013 (0.043)	0.042 (0.042)	-0.044 (0.047)	0.018 (0.048)	-0.094* (0.055)	-0.170*** (0.063)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	302	276	321	329	371	442	404	382	359	314
R-squared	0.040	0.034	0.063	0.021	0.020	0.036	0.008	0.023	0.055	0.063

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each income decile. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A9 Effect of the tax day on support for redistribution by income decile (clustered standard errors)

Income decile	1 st	2 nd	3 rd	4 th	5 th	6 th	7 th	8 th	9 th	10 th
Treatment	0.165 (0.207)	0.056 (0.348)	0.657** (0.233)	-0.114 (0.224)	-0.155 (0.154)	0.405** (0.173)	-0.216 (0.333)	0.110 (0.217)	-0.275 (0.198)	-0.998*** (0.239)
Days	-0.035 (0.024)	0.049 (0.042)	-0.085** (0.030)	0.021 (0.034)	0.016 (0.022)	-0.049** (0.020)	0.037 (0.038)	-0.022 (0.034)	0.071** (0.031)	0.157*** (0.026)
Treatment × Days	0.042 (0.029)	-0.095* (0.049)	0.050 (0.044)	-0.013 (0.039)	-0.013 (0.030)	0.042* (0.022)	-0.044 (0.047)	0.018 (0.042)	-0.094** (0.034)	-0.170*** (0.031)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	302	276	321	329	371	442	404	382	359	314
R-squared	0.040	0.034	0.063	0.021	0.020	0.036	0.008	0.023	0.055	0.063

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each income decile. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A10 Effect of the tax day on support for redistribution by age group

Age group (years)	15-24	25-34	35-44	45-54	55-64	65-74	75-100
Treatment	0.586** (0.237)	0.038 (0.265)	-0.272 (0.288)	-0.046 (0.270)	0.162 (0.259)	-0.155 (0.280)	-0.301 (0.428)
Days	-0.055* (0.032)	0.038 (0.036)	0.047 (0.039)	0.020 (0.036)	-0.046 (0.036)	0.037 (0.037)	0.031 (0.057)
Treatment \times Days	0.027 (0.035)	-0.087** (0.040)	-0.063 (0.042)	-0.018 (0.039)	0.072* (0.039)	-0.047 (0.042)	-0.038 (0.063)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓
Observations	530	544	590	625	698	495	307
R-squared	0.024	0.024	0.014	0.012	0.035	0.016	0.019

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each age group. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Table A11 Effect of the tax day on support for redistribution by age group (clustered standard errors)

Age group (years)	15-24	25-34	35-44	45-54	55-64	65-74	75-100
Treatment	0.586** (0.247)	0.038 (0.219)	-0.272* (0.134)	-0.046 (0.265)	0.162 (0.165)	-0.155 (0.228)	-0.301 (0.356)
Days	-0.055* (0.028)	0.038 (0.035)	0.047** (0.020)	0.020 (0.037)	-0.046 (0.029)	0.037 (0.033)	0.031 (0.048)
Treatment \times Days	0.027 (0.031)	-0.087** (0.039)	-0.063** (0.022)	-0.018 (0.041)	0.072** (0.031)	-0.047 (0.034)	-0.038 (0.052)
Survey-year FE	✓	✓	✓	✓	✓	✓	✓
Observations	530	544	590	625	698	495	307
R-squared	0.024	0.024	0.014	0.012	0.035	0.016	0.019

Data: ESS Finland 2002-18. *Note:* Estimates are from OLS regressions with robust standard errors clustered at the *Days* level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on support for redistribution for each age group. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

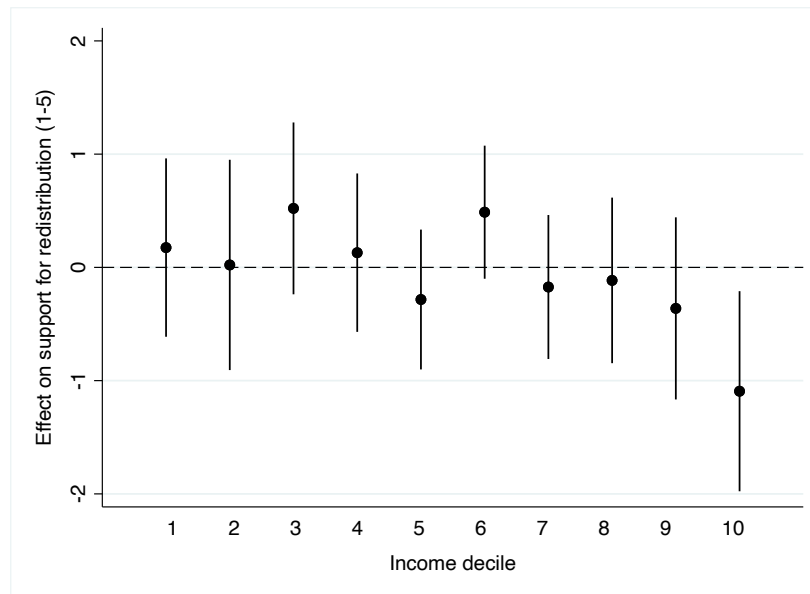
Table A12 Effect of the tax day on alternative measures of support for redistribution

	(1) Support for unemployed	(2) Support for childcare	(3) Pref. for econ. equality	(4) Support for safety net
Below-median income				
Treatment	-0.105 (0.648)	-0.192 (0.669)	-0.812 (1.147)	1.223 (0.787)
Days	-0.006 (0.102)	0.017 (0.105)	0.174 (0.134)	-0.134 (0.092)
Treatment × Days	0.006 (0.106)	-0.003 (0.109)	-0.207 (0.145)	0.115 (0.099)
Survey-year FE	✓	✓		
Observations	426	422	97	97
R-squared	0.018	0.030	0.061	0.025
Above-median income				
Treatment	0.503 (0.460)	0.333 (0.463)	1.298* (0.662)	-0.149 (0.528)
Days	-0.089 (0.070)	-0.072 (0.070)	-0.129* (0.076)	0.031 (0.061)
Treatment × Days	0.084 (0.074)	0.054 (0.074)	0.051 (0.089)	-0.034 (0.071)
Survey-year FE	✓	✓		
Observations	574	570	158	158
R-squared	0.003	0.011	0.036	0.004

Data: ESS Finland 2008, 2016, 2018. *Note:* Estimates are from OLS regressions with standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The *Treatment* coefficient captures the effect of the tax day on the outcome. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for the less affluent sub-sample (below-median income) and the bottom panel shows estimates for the affluent sub-sample (above-median income). Support for unemployment benefits captures the extent to which respondents think that it should be governments' responsibility to ensure a reasonable standard of living for the unemployed (1-not at all, 11-entirely). Support for public childcare captures the extent to which respondents think that it should be governments' responsibility to ensure sufficient child care services for working parents (1-not at all, 11-entirely). Preference for economic equality captures how much respondents agree or disagree with the statement that a society is fair when income and wealth are equally distributed among all people (1-disagree strongly, 5-agree strongly). Support for social safety net captures how much respondents agree or disagree with the statement that a society is fair when it takes care of those who are poor and in need regardless of what they give back to society (1-disagree strongly, 5-agree strongly).

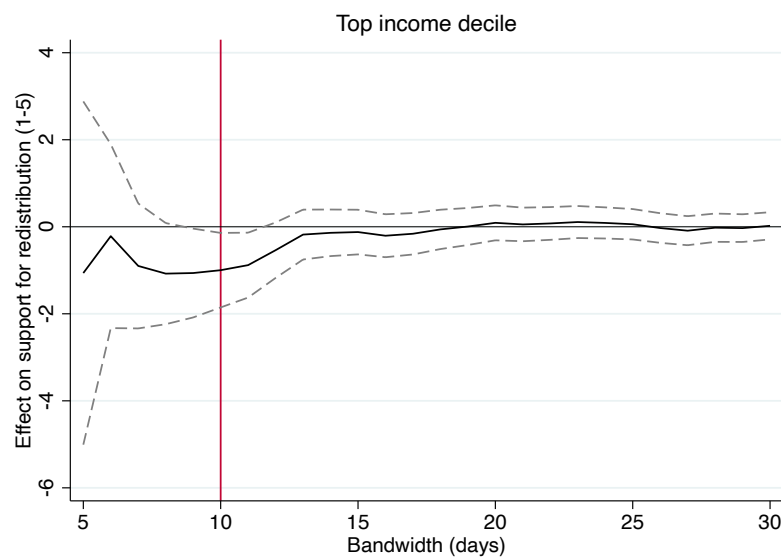
C Robustness checks

Fig. A8 Effect of the tax day on support for redistribution by income decile (with day-of-the-week fixed effects)



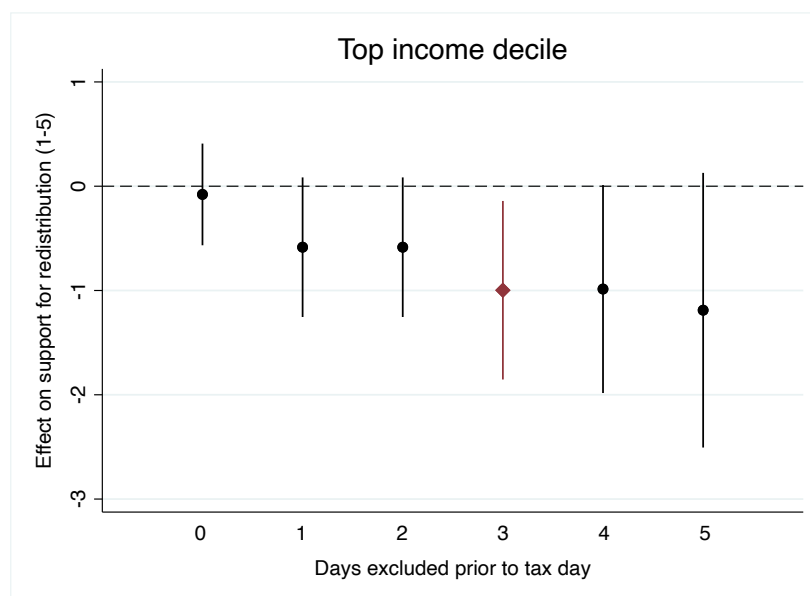
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Estimates are from our baseline model with 10-day bandwidths and day-of-the-week fixed effects, fitted separately on each income decile. Vertical lines represent 95% confidence intervals. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Fig. A9 Effect of the tax day on support for redistribution amongst the top income decile (varying bandwidths)



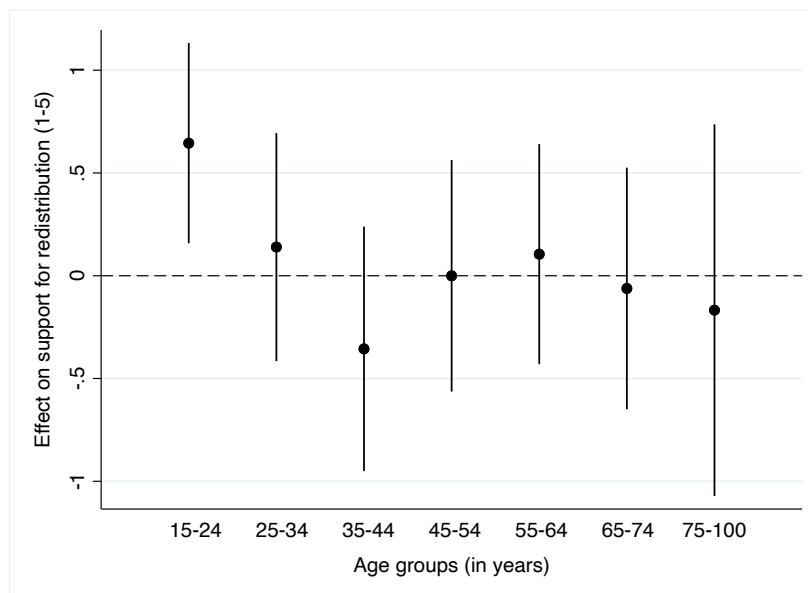
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on support for redistribution in the top income decile for varying bandwidths (days) around the tax day. Support for redistribution measures to extent to which respondents agree that the government should take measures the reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The red vertical line marks the default bandwidth of 10 days around the tax day.

Fig. A10 Effect of the tax day on support for redistribution amongst the top income decile (alternative exclusion windows prior to the tax day)



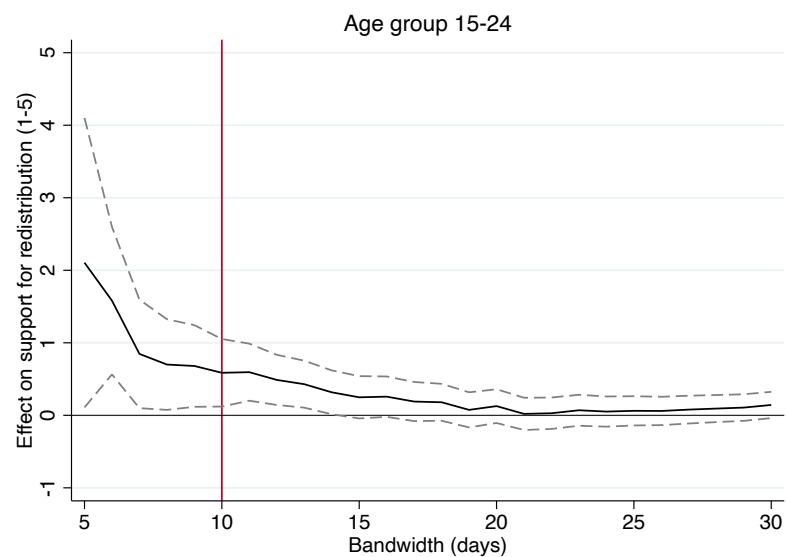
Data: ESS Finland 2002-18. *Note:* Point estimates (with 95% confidence intervals) show the effect of the tax day on support for redistribution depending on how many days prior to the event are excluded from the analysis. The default is 3 days and highlighted in red. Estimates are based on OLS with survey-year fixed effects. The sample is restricted to the top income decile and a 10-day window around the tax day in a given year. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Fig. A11 Effect of the tax day on support for redistribution by age group (with day-of-the-week fixed effects)



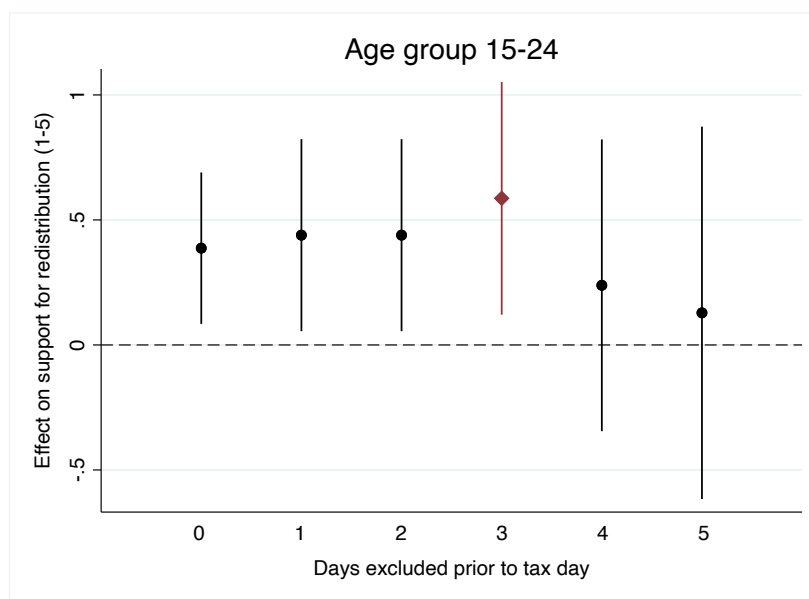
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 9-year bins. Estimates are from our baseline model with 10-day bandwidths and day-of-the-week fixed effects, fitted separately on each age group. Vertical lines represent 95% confidence intervals. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Fig. A12 Effect of the tax day on support for redistribution amongst the the youngest age group (varying bandwidths)



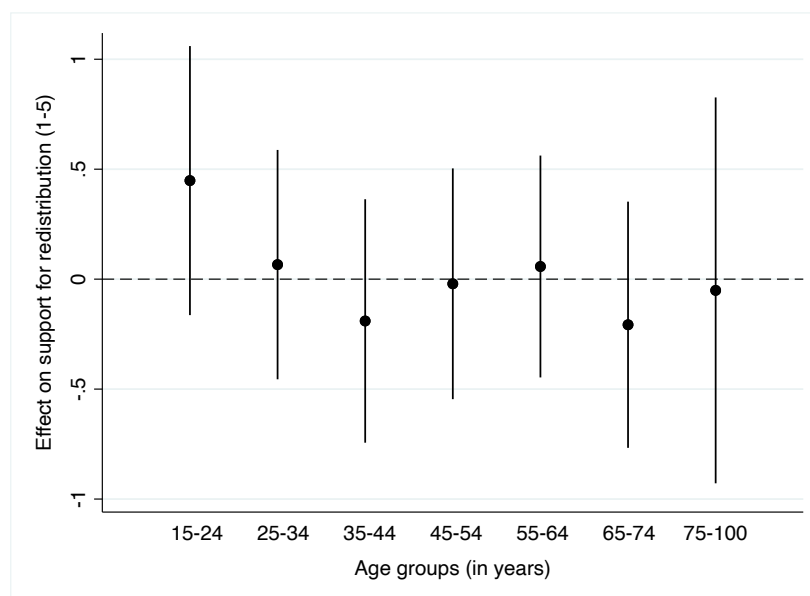
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on support for redistribution in the youngest age group (15-24 years) for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The red vertical line marks the default bandwidth of 10 days around the tax day.

Fig. A13 Effect of the tax day on support for redistribution amongst the youngest age group (alternative exclusion windows prior to the tax day)



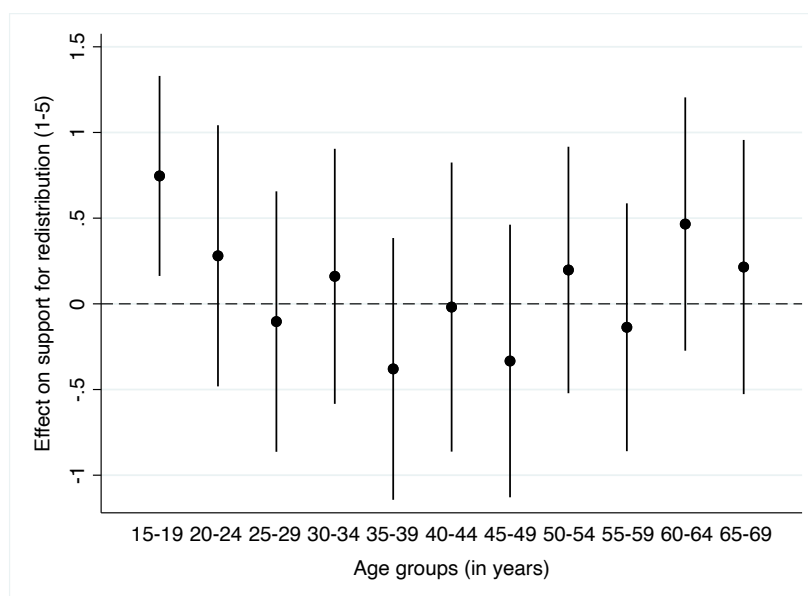
Data: ESS Finland 2002-18. *Note:* Point estimates (with 95% confidence intervals) show the effect of the tax day on support for redistribution depending on how many days prior to the event are excluded from the analysis. The default is 3 days and highlighted in red. Estimates are based on OLS with survey-year fixed effects. The sample is restricted to the youngest age group (15-25 years) and a 10-day window around the tax day in a given year. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Fig. A14 Effect of the tax day on support for redistribution by age group (controlling for household income)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, controlling for respondents' household income rank (in deciles). Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

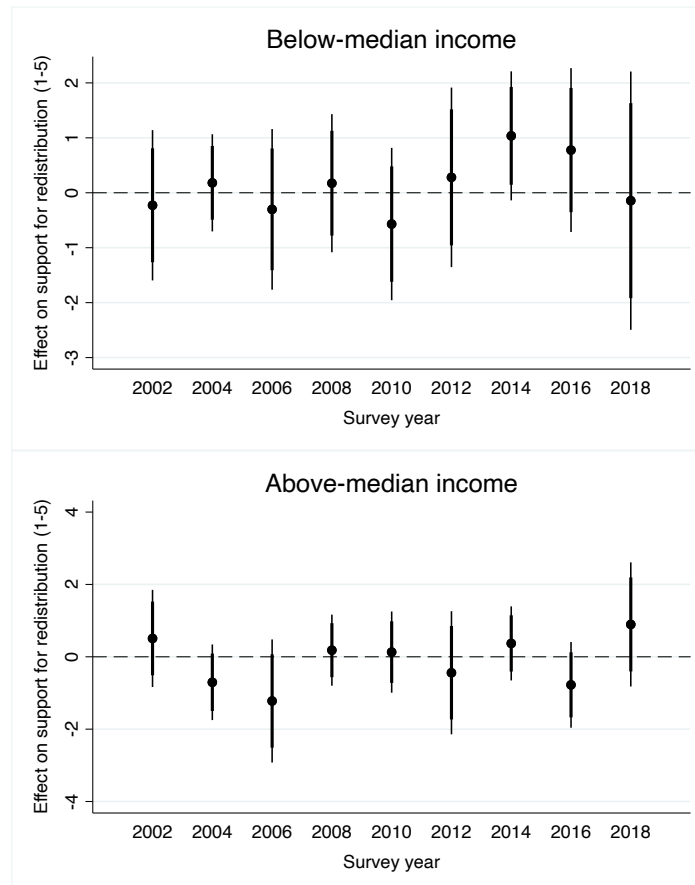
Fig. A15 Effect of the tax day on support for redistribution by age group (4-year bins)



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group, using 4-year bins. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

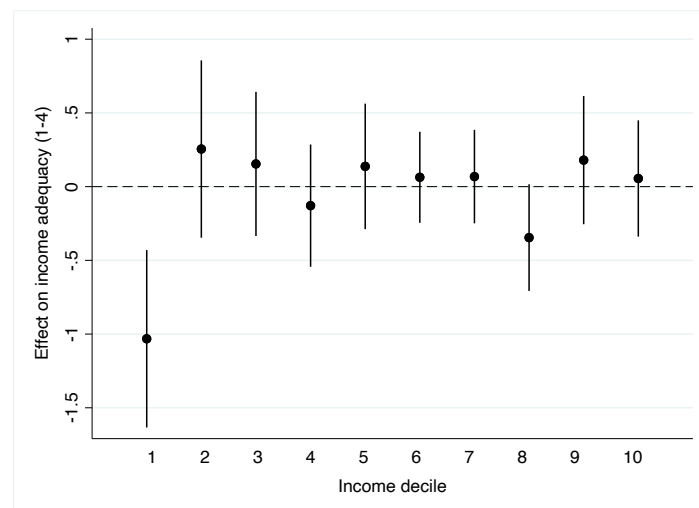
D Supplementary material on mechanisms

Fig. A16 Effect of tax day on support for redistribution (estimated separately for each survey year)



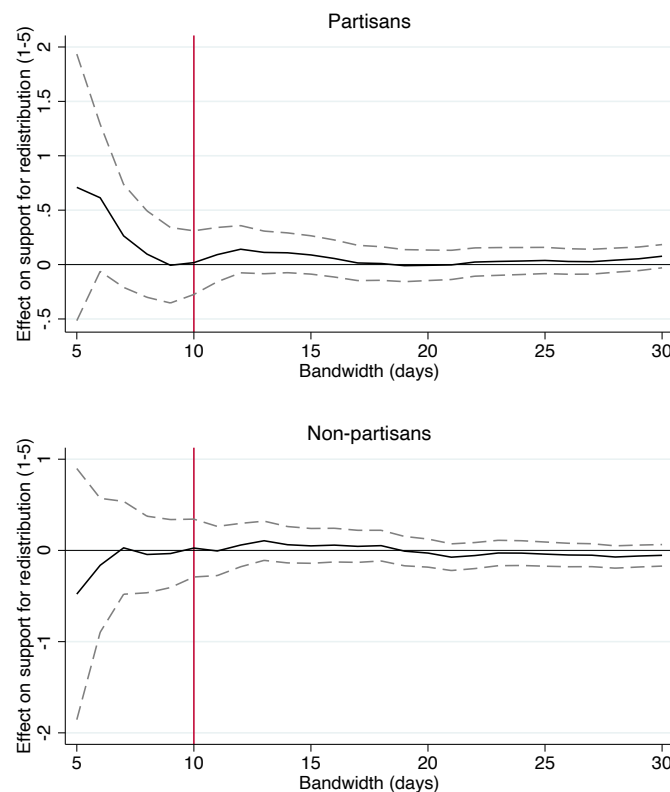
Data: ESS Finland 2002-18. *Note:* Point estimates show the effect of the tax day on support for redistribution, estimated separately for each available survey year using OLS. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The sample is restricted to a 10-day window around the tax day in a given year and the 3 days prior to the tax day are excluded. The top panel shows estimates for below-median income earners and the bottom panel shows estimates for the above-median income earners. Support for redistribution captures the extent to which respondents agree that the government should take measures to reduce differences in income levels (1-disagree strongly, 5-agree strongly).

Fig. A17 Effect of the tax day on perceived income adequacy by income decile



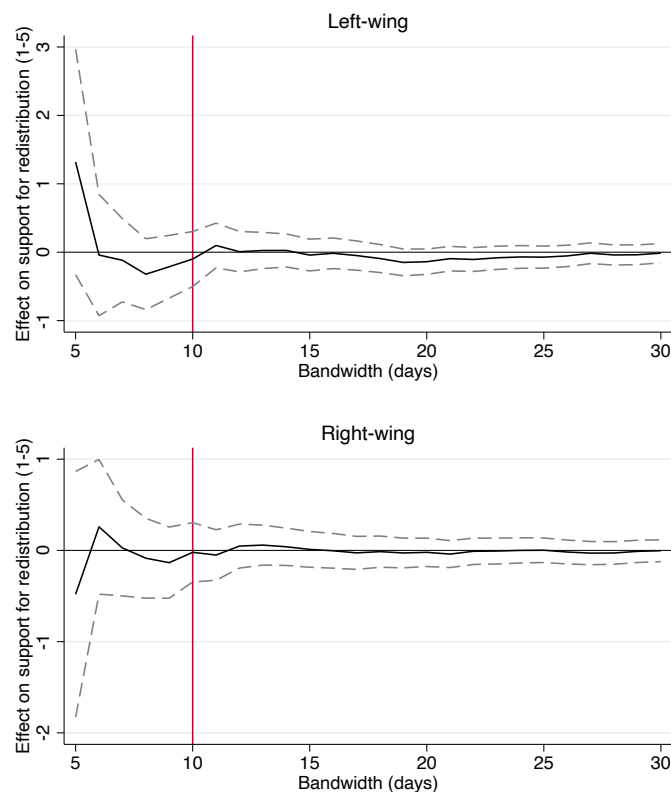
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perceived income adequacy by income decile. Vertical lines represent 95% confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Perceived income adequacy captures how respondents feel about their household's income nowadays, with response options ranging from 1 (finding it very difficult) to 4 (living comfortably).

Fig. A18 Effect of tax day on support for redistribution (partisans vs non-partisans)



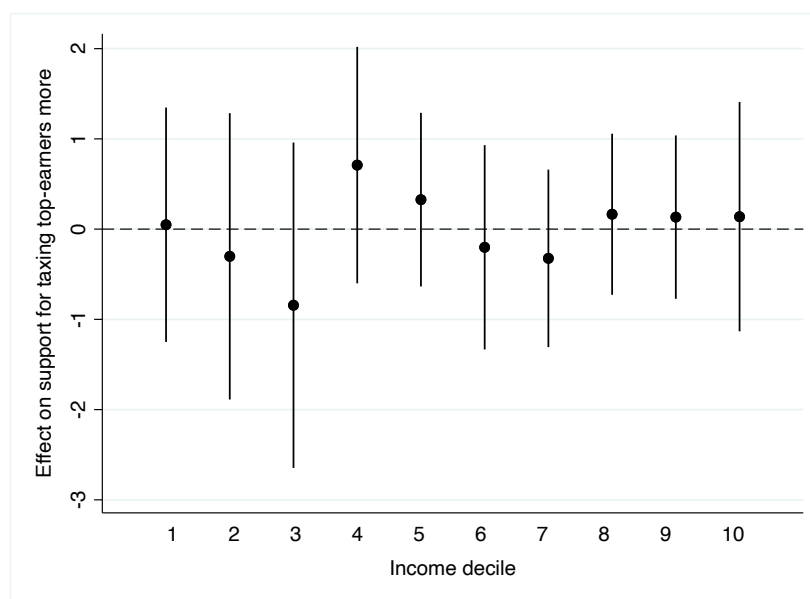
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for partisans (top panel) and non-partisans (bottom panel). Partisanship is measured the with the following item: "Is there a particular political party you feel closer to than all the other parties?" (Yes = 1; No = 0). The red vertical line marks the default bandwidth of 10 days around the tax day. All models control for individuals' household income rank (in deciles).

Fig. A19 Effect of tax day on support for redistribution (left- vs right-wing respondents)



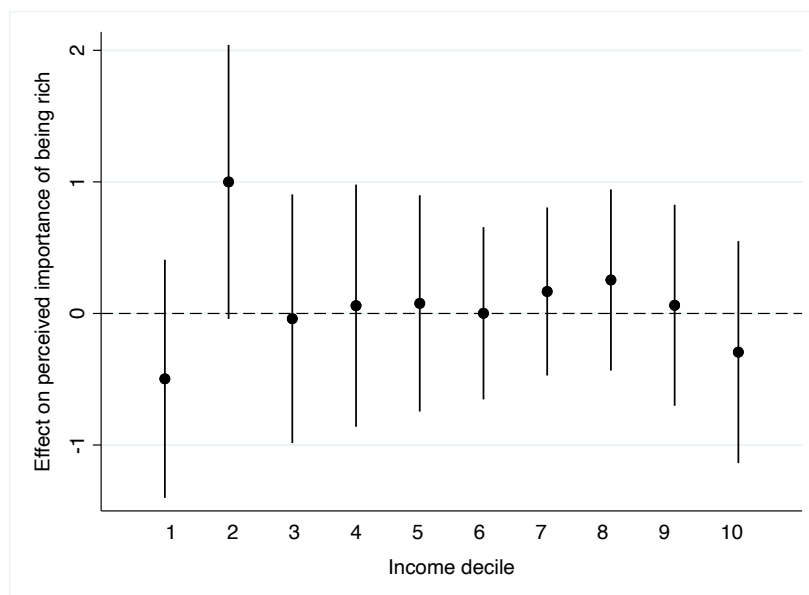
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for left-wing (top panel) and right-wing respondents (bottom panel). A respondent is left-wing if she scored 4 or lower on a 0-10 left-right self-placement scale, and right-wing if she score 6 or higher. The red vertical line marks the default bandwidth of 10 days around the tax day. All models control for individuals' household income rank (in deciles).

Fig. A20 Effect of the tax day on support for taxing high-earners more by income decile



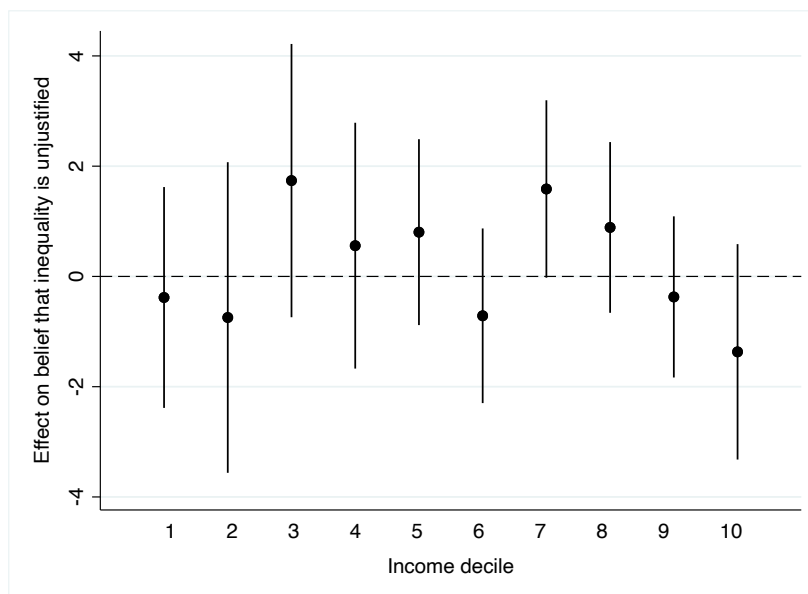
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for taxing top-earners more by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Support for taxing high-earners more is a binary variable which records whether respondents agree that "higher earners should pay a higher share (a higher %) of their earnings in taxes".

Fig. A21 Effect of the tax day on perceived importance of being rich by income decile



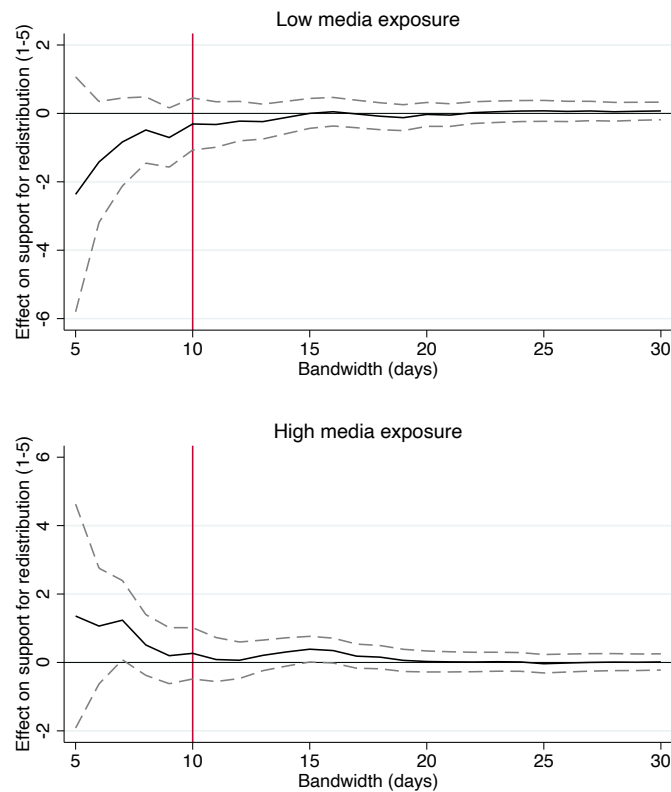
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' perception that being rich is important by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Perceived importance of being rich measures how much respondents' think they are like a person who values being rich, having money and expensive things. Response options range from 1 (very much like me) to 6 (not at all like me).

Fig. A22 Effect of the tax day on belief that inequality is unjustified by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' belief that inequality is unjustified by income decile. Vertical lines represent 95% confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. To increase precision of the estimates, we include controls for age, gender, education, and labour market status (in paid job, unemployed, student, retired, doing housework). Belief that inequality is unjustified measures respondents' support for the statement that "large differences in people's incomes are acceptable to properly reward differences in talents and efforts." Response options range from 1 (agree strongly) to 8 (disagree strongly) so that higher values reflect more left-wing beliefs.

Fig. A23 Effect of the tax day on support for redistribution (low vs high media exposure)



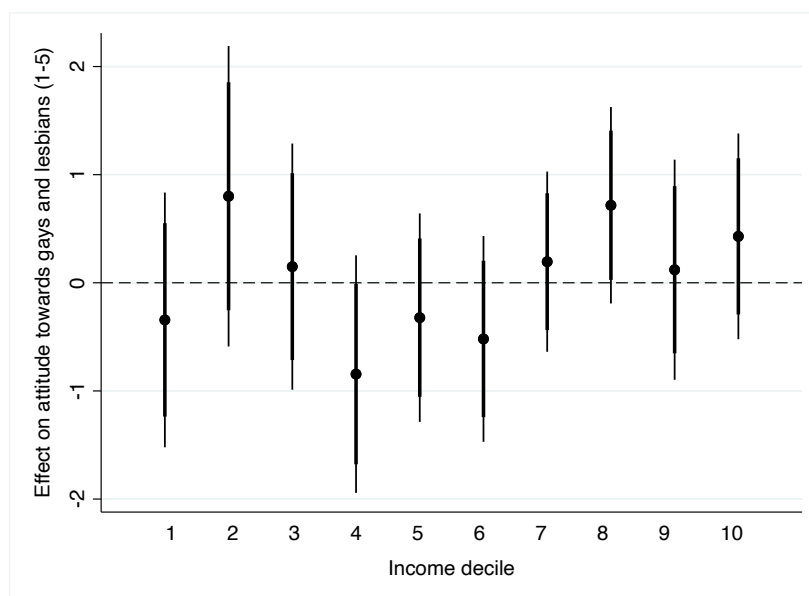
Data: ESS Finland 2018. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution for varying bandwidths (days) around the tax day. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly). The results are presented separately for respondents with high media exposure (>60 minutes per day) and low media exposure (<60 minutes per day). Media exposure refers to how much time respondents spend on a typical day watching, reading or listening to news about politics and current affairs (in minutes). The red vertical line marks the default bandwidth of 10 days around the tax day.

E Placebo tests

We conduct several placebo tests to assess the plausibility of the excludability assumption (see [Muñoz et al. 2020](#)). First, we show that the tax day has no significant effects on a placebo outcome (attitudes towards gays and lesbians) that is conceptually close to our main outcome of interest, but should in theory remain unaffected by the event.⁴² We find null effects regardless of whether we disaggregate the analysis by income decile (Figure [A24](#)) or age group (Figure [A25](#)). Second, to rule out that global shocks or time trends are behind our findings, we run the main analysis on ESS respondents from Sweden, who should in theory remain unaffected by the tax day. Reassuringly, we find null effects regardless of whether we disaggregate the analysis by income decile (Figure [A26](#)) or age group (Figure [A27](#)). Finally, to check for problematic time trends, we split the control group sub-sample (who were interviewed before the tax day) at its empirical mean (23 days before the tax day) and test for the absence of a placebo effect at that point (see [Muñoz et al. 2020](#)). Again, we find null effects regardless of whether we disaggregate the analysis by income decile (Figure [A28](#)) or age group (Figure [A29](#)).

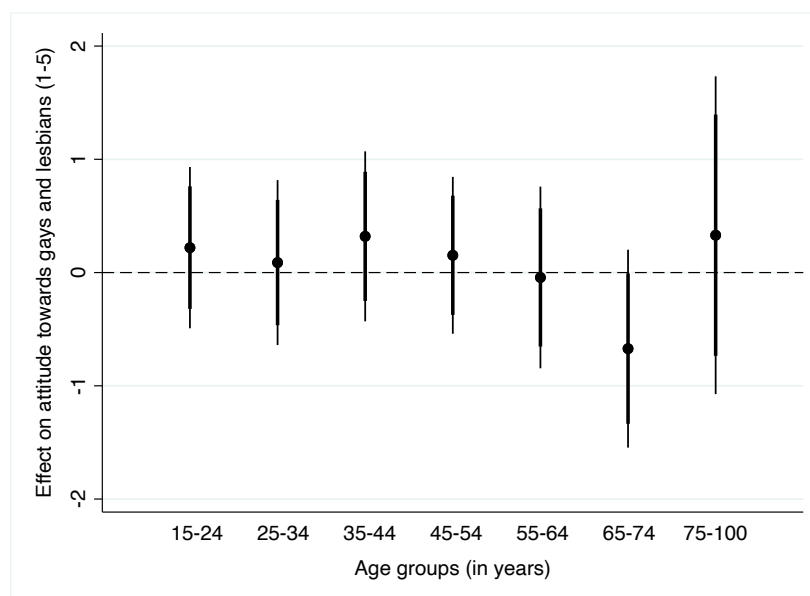
⁴²Support for redistribution reflects the classical left-right dimension in politics, whilst attitudes towards gays and lesbians taps into the “second” dimension of socio-cultural politics.

Fig. A24 Placebo test - Effect of the tax day on attitude towards gays and lesbians by income decile



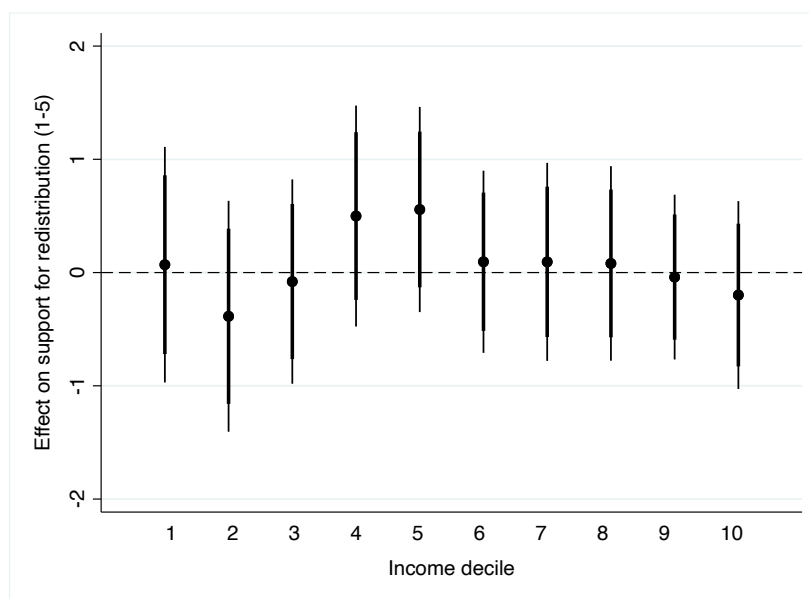
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' attitude towards gays and lesbians by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Attitude towards gays and lesbians measures respondents' agreement with the statement that "gay men and lesbians should be free to live their own life as they wish". Response options range from 1 (Disagree strongly) to 5 (Agree strongly).

Fig. A25 Placebo test - Effect of the tax day on attitude towards gays and lesbians by age group



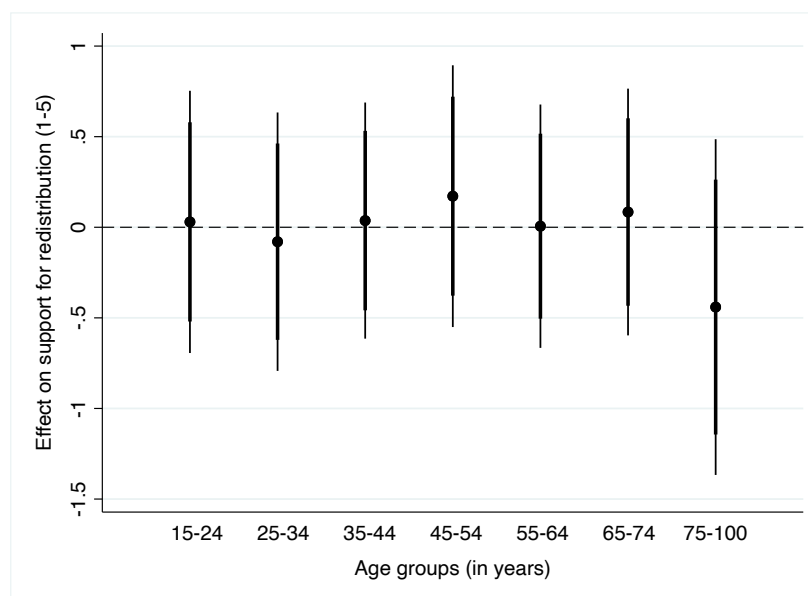
Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on respondents' attitude towards gays and lesbians by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from an OLS model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Attitude towards gays and lesbians measures respondents' agreement with the statement that "gay men and lesbians should be free to live their own life as they wish". Response options range from 1 (Disagree strongly) to 5 (Agree strongly).

Fig. A26 Placebo test - Effect of the tax day on support for redistribution amongst Swedish ESS respondents (by income decile)



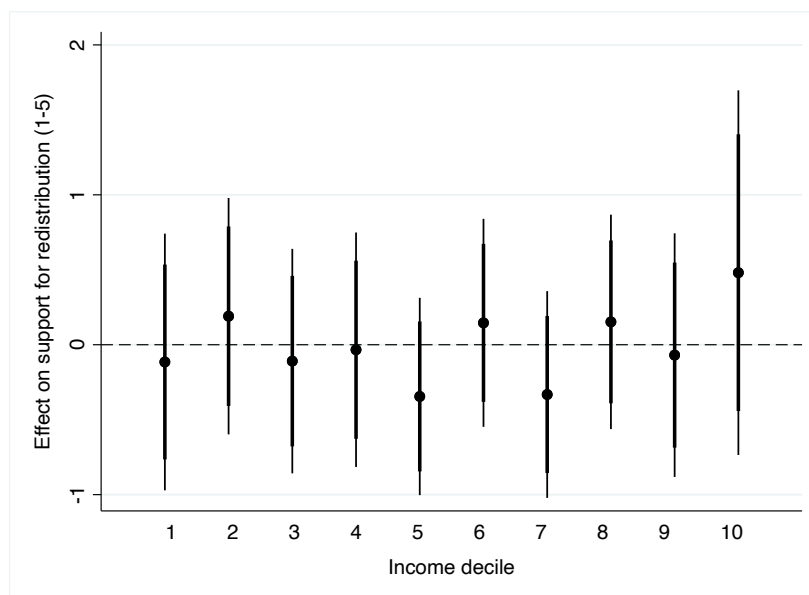
Data: ESS Sweden 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on Swedish respondents' support for redistribution by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Fig. A27 Placebo test - Effect of the tax day on support for redistribution amongst Swedish ESS respondents (by age group)



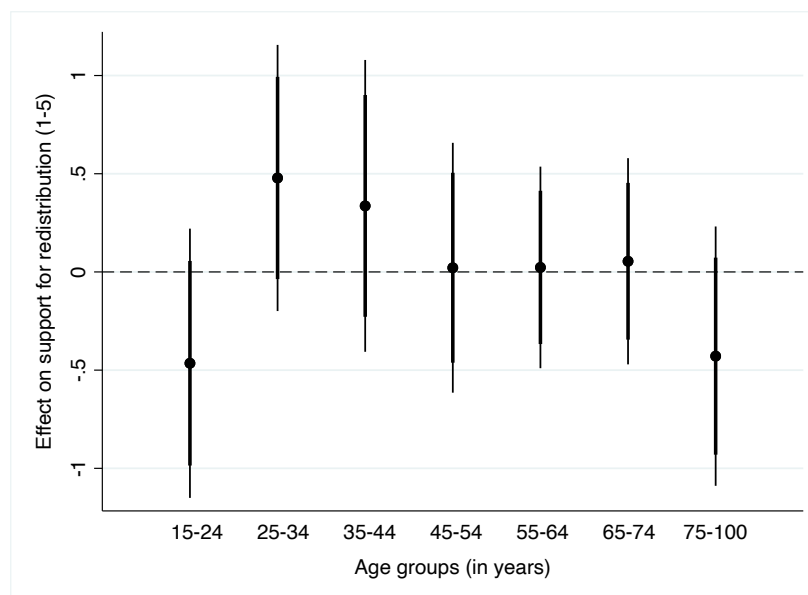
Data: ESS Sweden 2002-18. *Note:* The graph shows the estimated effect of the tax day (the coefficient on the *Treatment* indicator) on Swedish respondents' support for redistribution by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Fig. A28 Placebo test - Effect of fake tax day on support for redistribution by income decile



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of a fake tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by income decile. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The fake tax day is located at the empirical mean (23 days before the tax day) of the control group (who were interviewed before the tax day). The sample is restricted to respondents in the control group. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each income decile. The three days prior to the fake tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Fig. A29 Placebo test - Effect of fake tax day on support for redistribution by age group



Data: ESS Finland 2002-18. *Note:* The graph shows the estimated effect of a fake tax day (the coefficient on the *Treatment* indicator) on respondents' support for redistribution by age group. Vertical lines represent 95% (thick) and 99% (thin) confidence intervals. The fake tax day is located at the empirical mean (23 days before the tax day) of the control group (who were interviewed before the tax day). The sample is restricted to respondents in the control group. Estimates are from our baseline model with 10-day bandwidths, fitted separately on each age group. The three days prior to the fake tax day are excluded from the analysis. Support for redistribution measures the extent to which respondents agree that the government should take measures to reduce differences in income levels, ranging from 1 (disagree strongly) to 5 (agree strongly).

Chapter 3

Does enfranchisement improve citizens' political maturity? Evidence from voting at 16 in Germany

Abstract: Historically, women, racial minorities, and the working class were excluded from voting on the basis of their supposedly insufficient political maturity. The same argument is currently used by opponents of enfranchising 16-year-olds. An implicit assumption underlying this argument is that citizens acquire political maturity independently of whether or not they have the right to vote. I examine whether enfranchisement affects citizens' political maturity by leveraging a quasi-experiment in Germany, where states enfranchised 16-year-olds at different times, starting in 1996. The results show that enfranchising 16-year-olds can equalise prior differences in political maturity between underage and adult youth – measured by their political interest, efficacy, willingness to vote, and attitudinal consistency. This effect appears to be driven by an increase in demand for political information amongst the newly enfranchised. The findings make it more difficult to argue against enfranchising 16-year-olds on the basis of their supposedly insufficient political maturity, as political maturity itself appears to be affected by the right to vote.

Keywords: Enfranchisement, voting at 16, young people, political maturity

3.1 Introduction

Historically, one of the main arguments used to justify the exclusion of women, racial minorities, and the working class from the franchise was that these groups are not politically mature enough to vote – that they lack political interest and knowledge, or have inconsistent political attitudes (Corder and Wolbrecht, 2006; Keyssar, 2009; Mill, 1861; Valelly, 2009).¹ Nowadays, the same argument is used in the debate over lowering the voting age to 16, which is on-going in many Western democracies (Eichhorn and Bergh, 2020).² In striking parallels to the historical debates, critics of voting at 16 worry that franchise extension could reduce overall turnout by adding to the electorate a group of voters with a low propensity to vote (Commission, 2004; McAllister, 2014). Furthermore, critics worry that 16-17 year-olds would make less informed vote choices than adult citizens (Chan and Clayton, 2006).³ An implicit assumption underlying these arguments is that citizens acquire political maturity independently of whether or not they have the right to vote. However, recent research suggests that voting or being entitled to vote can influence citizens' downstream attitudes and behaviours (Braconnier et al., 2017; Ferwerda et al., 2020; Mullainathan and Washington, 2006; Shineman, 2016), so citizens' political maturity may also be a function of their right to vote. This raises the question whether enfranchisement can improve citizens' political maturity.

I address this question by studying a quasi-experiment in Germany, where several states enfranchised 16-17 year-olds (“underage youth”) in municipal and state elections at different times over the past decades. The empirical strategy relies on nationally representative survey data and a triple-difference approach, where already enfranchised 19-20 year-olds (“adult youth”) and states that did not introduce voting at 16 are used as control groups. The results provide evidence that enfranchisement can equalise prior maturity differences between underage and adult youth – measured by their interest in politics, internal and external efficacy, willingness to vote, and attitudinal consistency.⁴ While disenfranchised 16-17 year-olds are on average less interested in politics than adult youth, these differences are reduced by more than one third when voting at 16 is implemented. Even stronger equalising effects are observed for willingness to vote and internal efficacy, where prior differences between

¹Political maturity is the term most frequently used by critics of franchise extension. Alternatives are voter preparedness and political capacity (Lau, 2012)

²In the US, several municipalities already allow 16-year-olds to vote. Voting at 16 has also been implemented in Argentina, Austria, Brazil, Ecuador, Malta, and Nicaragua at the national level, and Estonia, Germany, Norway and the UK at the sub-national level.

³A related concern is that 16-17 year-olds are more supportive of anti-establishment parties than adults (Bronner and Ifkovits, 2019).

⁴Other measures such as political knowledge are unfortunately not available.

underage and adult youth largely disappear when voting at 16 is implemented. Voting at 16 also appears to have equalising effects on external efficacy, but the results are less robust. Finally, I find no significant difference in attitudinal consistency between disenfranchised 16-17 year-olds and adult youth, and the introduction of voting at 16 does not change this.

Exploring potential mechanisms, I find that an increase in demand for political information amongst the newly enfranchised may explain why voting at 16 equalises maturity differences between underage and adult youth. I also show that this demand effect applies primarily to the first cohort of enfranchised 16-17 year-olds, which indicates that political education initiatives implemented alongside the reform are crucial for ensuring that enfranchisement results in improved outcomes. Overall, the findings offer an important rebuttal to the critics of voting at 16 – albeit with a caveat. On the one hand, they cast doubts on the implicit assumption behind the critics’ argument, which is that citizens acquire political maturity independently of whether or not they can vote. On the other hand, the results suggest that enfranchisement alone is unlikely to lead to improved outcomes, as education initiatives implemented alongside the reform may be crucial for enabling 16-17 year-olds to catch up with their slightly older peers.

3.2 Related literature

The main contribution of this paper is to provide causal evidence showing that enfranchisement can equalise maturity differences between 16-17 year-olds and adult youth. Previous studies on voting at 16 have attempted to show that 16-17 are (not) politically mature enough to vote by employing data on disenfranchised 16-17 year-olds ([Chan and Clayton, 2006](#); [Mahéo and Bélanger, 2020](#); [Stiers, Hooghe and Goubin, 2020](#)) or extrapolating from data on enfranchised adults ([McAllister, 2014](#)). However, these studies have failed to address the relevant counterfactual, which is how politically mature 16-17 year-olds would be if they had the right to vote. Recent studies from Scotland, Austria, and Norway – where voting at 16 has already been implemented – are in a better position to address this ([Bergh, 2013](#); [Eichhorn, 2017](#); [Wagner et al., 2012](#); [Zeglovits, 2013](#)). However, the results have so far been inconclusive and credible causal conclusions have not been possible, as these studies primarily rely on simple cross-sectional or before-and-after research designs.

[Eichhorn \(2017\)](#) uses UK-wide survey data from 2015 to argue that the enfranchisement of 16-17 year-olds in the 2014 Scottish independence referendum improved their political maturity in comparison to the same age group in the rest of the UK. Unfortunately, there are no pre-referendum baseline data, so the observed post-referendum differences cannot

be attributed to enfranchisement. Besides Eichhorn's 2017 study of the 2014 Scottish independence referendum, recent research on voting at 16 has focused on the cases of Austria and Norway. Zeglovits and Zandonella (2013) use survey data from Austria to argue that enfranchisement increased the political interest of 16-17 year-olds. However, the study compares 16-17 year-olds before and after the 2007 voting age reform, which makes it difficult to attribute the observed difference in political interest to enfranchisement, as cohort differences might explain some of the variation. Wagner et al. (2012) also use survey data from Austria to show that there are no significant differences in political interest and quality of vote choice between enfranchised 16-17 year-olds and older age groups. However, they cannot assess whether this is the result of enfranchisement, as they do not have pre-reform baseline data. Furthermore, their null results may be due to the small sample size ($n = 263$ for 16-25 year-olds). Bergh (2013) uses data from the 2011 Norwegian voting age trial and finds no evidence that enfranchising 16-year olds improves their political knowledge, interest or efficacy. However, Norwegian municipalities needed to apply to participate in the trial, and only 20 were selected by the government – in part because they pursued active youth policies – so the results may suffer from selection bias.

Finally, Stiers, Hooghe and Goubin (2020) employ a regression discontinuity design and survey data collected in the wake of a 'mock' election organised for 16-17 year-olds in the Belgian city of Ghent. The authors find that 'mock' enfranchisement of 16-17 year-olds has zero effects on several measures of political engagement (reported frequency of political discussions, political knowledge, internal and external efficacy, and political trust), but a small positive effect on reported attention to politics. However, it remains unclear whether the null results are simply due to the fact that this was a 'mock' election and most 16-17 year-old respondents knew that their votes would not count. Furthermore, it is possible that the null findings are driven by the relatively small sample size ($n = 2360$; the effective number of observations used around the RD cut-off is not reported) or the non-representative nature of the sample, which is presumably mostly composed of adolescents who are already politically engaged (Stiers, Hooghe and Goubin, 2020).⁵ Whether enfranchising 16-17 year-olds affects their political maturity thus remains a relatively open question in the literature on voting at 16.

A second contribution of the paper relates to the on-going debate over the transformative potential of voting. While several studies find that voting can affect second-order outcomes such as political interest, knowledge, polarisation and party identification (Braconnier et al.,

⁵The survey response rate was only 21.62 percent. The study used a type of snowball sampling procedure, whereby the youngest sibling of an eligible family was asked to pass the survey questionnaires to any older siblings.

2017; Bronner and Ifkovits, 2019; Ferwerda et al., 2020; Shineman, 2016), others find no evidence for the transformative voting hypothesis (De Leon and Rizzi, 2014; Holbein and Rangel, 2020; Holbein et al., 2020; Rosenqvist, 2017). Importantly, previous studies have relied on small-n field experiments (Braconnier et al., 2017; Loewen et al., 2008; Shineman, 2016) and regression discontinuity designs that focus on narrow sub-samples of respondents around the voting-age threshold (De Leon and Rizzi, 2014; Holbein et al., 2020; Rosenqvist, 2017). This paper provides novel evidence in support of the transformative voting hypothesis in the context of a large-scale voting age reform.

3.3 Voting at 16 in Germany

Several German states introduced voting at 16 for municipal and state elections at different times, starting in 1996. This quasi-experiment can be leveraged to isolate the causal effect of enfranchising 16-year-olds. Figure 1 provides an overview of the introduction of voting at 16 (see Appendix A for a timeline). The national voting age remains 18 years. State and municipal elections take place every five years, although not necessarily at the same time.⁶ Electoral systems differ between states, but most states use a personalised closed-list proportional representation system (“personalisierte Verhältniswahl”) for state elections⁷ and an open-list proportional representation system for municipal elections. In state elections, parties need to garner at least 5% of votes in order to obtain a seat. There is no equivalent threshold in municipal elections. German citizens above the legal voting age are eligible to vote in a state election if they have their primary residence in the state for at least three months. In municipal elections, EU citizens also have the right to vote provided they have their primary residence in the municipality for at least three months.⁸

⁶In Bremen elections take place every four years. Municipal elections often take place at the same time as EU elections. All dates for previous state elections are available at: <https://www.bundeswahlleiter.de/service/landtagswahlen.html>. Dates for previous municipal elections are found on the websites of the Returning Officer of each state (Landeswahlleiter). Available at: <http://www.wahlrecht.de/links.htmwl>.

⁷As for national elections, citizens cast two votes: The first vote (Erststimme) is for a direct candidate, who is elected by a simple plurality, and the second vote (Zweitstimme) is for a party list, which determines the final distribution of seats in the legislature. For an overview (in German) see <https://www.bpb.de/politik/wahlen/wahlen-in-deutschland/335656/wahlsysteme>

⁸For details on state election laws see <https://www.wahlrecht.de/landtage/>. For details on municipal election laws see <http://www.wahlrecht.de/kommunal/>.

Voter registration in Germany is automatic and turnout is typically between 55-75% in state elections and between 40-60% in municipal elections.⁹ Reliable turnout data for 16-17 year-olds are difficult to obtain as German law prohibits the distribution of electoral data at low levels of aggregation.¹⁰ However, evidence from Brandenburg, Bremen and Hamburg suggests that turnout amongst enfranchised 16-17 year-olds is typically higher than amongst slightly older adult youth and very similar to overall turnout rates. As of 2019, there were around 1.4 million German citizens aged 16-17 years, and if enfranchised, they would make up around 2.3% of the electorate of 61.7 million. Around 80% of 16-17 year-olds still go to school and 10% are trainees (Vehrkamp et al., 2015).

Why did some states introduce voting at 16 whilst others did not? Available evidence suggests that constitutional constraints and party politics played a key role. In 11 states, the voting age for state elections has constitutional status, so it can only be changed by a two-thirds majority in the state legislature or a referendum (Leininger and Faas, 2020). Only in Hessen, Hamburg, Bremen, Mecklenburg-Vorpommern, and Schleswig-Holstein can the voting age for state elections be changed with a simple majority – which was done in the latter four cases. The voting age for municipal elections typically does not have constitutional status and can be changed by a simple majority. With the exceptions of Brandenburg, Berlin and Thüringen, voting at 16 has only been implemented in states and for elections where the voting age could be changed with a simple majority (Leininger and Faas, 2020). Past legislative initiatives to lower the voting age also reveal a clear left-right divide on the issue, with Die Grünen and Die Linke supporting reform, SPD and FDP taking ambivalent positions, and CDU/CSU and AfD opposing reform. Consequently, voting at 16 has primarily been implemented in states that were governed by left-wing coalitions (Leininger and Faas, 2020).

3.4 Data and methods

The main data source is the Jugendsurvey (JS) and its successor Aufwachsen in Deutschland: Alltagswelten (AIDA).¹¹ Both are nationally representative, repeated cross-sections that track the social and political development of young people in Germany. The JS was implemented via face-to-face interviews in 1992, 1997 and 2003, with each wave reaching approximately

⁹Turnout data for state elections are available at: <https://www.bundeswahlleiter.de/service/landtagswahlen.html>. Turnout data for municipal elections are found on the website of the Landeswahlleiter of each state (<http://www.wahlrecht.de/links.htmwl>).

¹⁰Articles 1-8 WStatG, see <https://www.bundeswahlleiter.de/service/glossar/w/wahlstatistik.html>

¹¹The JS and AIDA are implemented by the German Youth Institute, which is funded by the German Federal Government (<https://www.dji.de/>)

Fig. 3.1 Staggered introduction of voting at 16 in Germany



Note: Years refer to the first election year with voting at 16. If a state introduced voting at 16 for municipal and state elections at different times, the first election with voting at 16 is used.

seven thousand respondents aged 16-29. AIDA was implemented via telephone and face-to-face interviews in 2009 and 2013-15, reaching 7.4 and nine thousand respondents aged 16-29 respectively.¹² As a robustness check, I also use data on political interest from the German Socio-Economic Panel (SOEP) (see Appendix F). For the main analysis, only respondents aged 16-17 (underage youth) and 19-20 (adult youth) are retained, and non-Germans are excluded.

¹²Response rates: wave 1 (65%), wave 2 (59.6%), wave 3 (48.6%), wave 4 (50.3%, 0-24 year-olds, 32.8% 25-32 year-olds), wave 5 (44.8%, U33 panel).

The key explanatory variable is an indicator equal to one for respondents in states and survey years where voting at 16 has been implemented.¹³ I employ five different measures of political maturity, all of which have been used in previous studies on voting at 16 (see Appendix B). Respondents' interest in politics, external efficacy (the belief that government will respond to one's demands), internal efficacy (the belief that one can understand politics and therefore participate), willingness to vote in a hypothetical election,¹⁴ and attitudinal consistency (on gender equality). The variables are described in detail in Appendix C. Table 3.1 presents descriptive statistics.

Table 3.1 Descriptive statistics

	Dataset	Years	Obs.	Mean	SD	Min	Max	Var type
Interest in politics	JS/AIDA	1992-2015	11815	2.78	1.00	1.00	5.00	Ordinal
External efficacy	JS	1992-2003	6976	0.00	1.00	-1.81	3.51	Cont.
Internal efficacy	JS	1992-2003	6976	0.00	1.00	-2.99	3.76	Cont.
Willingness to vote	JS	1992-2003	6990	0.88	0.33	0.00	1.00	Binary
Attitud. consistency	JS/AIDA	1992-2009	9400	0.50	0.50	0.00	1.00	Binary

Note: German respondents aged 16-17 and 19-20 years. Efficacy scores are standardised and obtained using PCA on the following items: (a) I don't think that politicians worry too much about what people like me think; (b) I understand a lot about politics; (c) People like me have no influence one way or the other on what the government does; (d) Politicians are only interested in getting elected and not in what the voters really want; (e) Sometimes I think politics is so complicated that a normal person can hardly understand it; (f) In our society there are only a few powerful people; everyone else has no influence on what the government does. Responses to the efficacy items are measured on a 6-point agree-disagree scale.

To isolate the causal effect of voting at 16, I employ a triple-difference approach, using underage youth (16-17 years) in reforming states as the treatment group, and already enfranchised adult youth (19-20 years) and states that did not introduce voting at 16 as control groups. Intuitively, the staggered introduction of voting at 16 lends itself to a difference-in-differences (DID) analysis, where state-level fixed effects account for unobserved time-invariant characteristics of states and year fixed effects account for unobserved time trends shared across states. However, time-varying state-level confounders are problematic in the DID design, as they violate the parallel trends assumption (Wing, Simon, and Bello-Gomez 2018). In Appendix G, I show that the parallel trends assumption would likely be violated in the simple DID design.

In our setting, one potential time-varying state-level confounder is the political party in control of the state legislature. Given that school curricula and public education spending

¹³If a state introduced voting at 16 for municipal and state elections, the first election with voting at 16 is used.

¹⁴This is a more suitable measure than turnout in real elections, given that some respondents aged 16-17 cannot vote

are determined at the state-level, there is a concern that both the voting age reforms as well as the political maturity of young people are co-determined by the party in power. Another concern is that political mobilisation for voting age reforms differs across states and time, which could affect both the likelihood of reform and young people's political maturity. To address these concerns, I use an additional within-state control group (19-20 year-olds) that is not affected by the reform but is plausibly exposed to the same problematic confounders (Muralidharan and Prakash, 2017). 18-year-olds are excluded to make sure that the older control group does not include any "treated" individuals. This is a concern because 18-year-olds typically attend the same class as 17-year-olds (who are in the treatment group), and the German voting age reforms were accompanied by school-based political education initiatives targeting the newly enfranchised. 18-year-olds are therefore likely to have been exposed to enfranchisement-related political education initiatives, either directly or indirectly through discussions with classmates. I discuss this further in the Section 3.6 and present results with 18-year-olds in Appendix F. Several robustness checks support the identifying assumption of parallel trends in this setup (Appendix F).

In the baseline specification, the effect of enfranchising 16-17-year-olds is estimated by

$$Y_{igst} = \gamma_s + \lambda_t + \delta_g + D_{st} + \beta(D_{st} * \delta_g) + (\gamma_s * \lambda_t) + X_{igst} + \varepsilon_{igst} \quad (3.1)$$

where Y_{igst} is a measure of individuals' political maturity, with higher values denoting greater political maturity, γ_s is a state-level fixed effect, λ_t is a survey-year fixed effect, δ_g is an indicator equal to one if the respondent is aged 16-17 years and zero if she is aged 19-20 years at the time of the survey interview, D_{st} is a treatment indicator equal to one for respondents in states and survey years where voting at 16 has been implemented,¹⁵ $\gamma_s * \lambda_t$ accounts for unobserved state-year specific shocks, and X_{igst} is a vector of time-varying state-level and individual-level controls (used in robustness checks). The quantity of interest is β , which identifies the marginal effect of enfranchisement on the political maturity of 16-17 year-olds relative to 19-20 year-olds. I then further relax the model and estimate a fully saturated triple-difference specification.

$$Y_{igst} = \gamma_s + \lambda_t + \delta_g + D_{st} + \beta(D_{st} * \delta_g) + (\gamma_s * \lambda_t) + (\gamma_s * \delta_g) + (\lambda_t * \delta_g) + X_{igst} + \varepsilon_{igst} \quad (3.2)$$

¹⁵This means that the treatment group includes 16-17 year-olds who could already participate in an election and 16-17 year-olds who are enfranchised, but could not yet exercise their right to vote.

Excluding the additional pair-wise interactions in the baseline specification is justified for statistical as well as theoretical reasons. First, the inclusion of the additional pair-wise interactions in Model 3.2 increases the standard error of β relative to Model 3.1, but in most cases does not affect the size of the coefficient β (see Appendix H). This indicates that lack of power is the main reason that results from Model 3.2 do not reach conventional significance levels. Indeed, the low level of statistical power is a key limitation of the research design, which relies on variation across only 16 states. Second, the exclusions can be theoretically justified. Unobserved time trends that are common across all states (e.g. national political campaigns) are unlikely to affect underage and adult youth differently, so the interaction between λ_t and δ_g may not be necessary. Similarly, unobserved time-invariant characteristics of states (e.g. Bavaria's unique political culture) are unlikely to affect underage and adult youth differently, so the interaction between γ_s and δ_g may also not be necessary. The exclusions are supported by F-tests, which suggest that the additional pair-wise interactions do not add explanatory power to the model (see Appendix H). All models are estimated using OLS with robust standard errors clustered at the state-level.

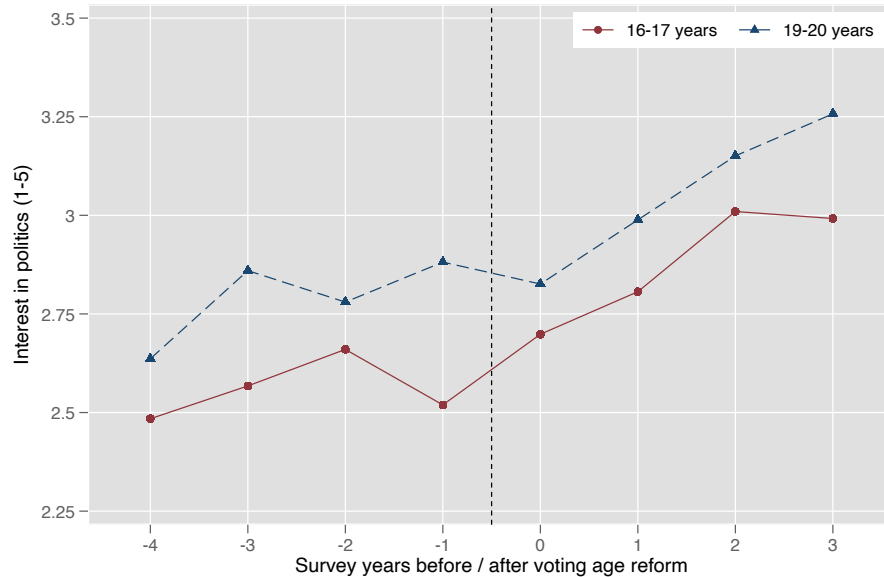
3.5 Results

Figure 3.2 plots the average interest in politics amongst underage and adult youth for all survey years before and after the voting age reform.¹⁶ The sample is restricted to states that switched to voting at 16 and the data are arranged so that year zero refers to the first survey year after reform. The reform coincides with an increase in political interest amongst 16-17 year-olds, both in absolute terms and relative to the older control age group. However, differential trends prior to reform caution against a causal interpretation. To isolate the causal effect of enfranchisement, I therefore include an additional control group: states that did not implement voting at 16.¹⁷ Results from the triple-difference type analysis are summarised in Table 3.2.

¹⁶Trend plots for other outcomes are in Appendix D.

¹⁷A trend plot for non-switching control states is in Appendix D.

Fig. 3.2 Voting age reform coincides with a (relative and absolute) increase in political interest amongst 16-17 year-olds



Note: The plot shows the average interest in politics amongst 16-17 year-olds (red dots) and 19-20 year-olds (blue triangles) for all survey years before and after the voting age reform (vertical dashed line). The sample is restricted to reforming states and the data are arranged such that year zero refers to the first survey year after reform.

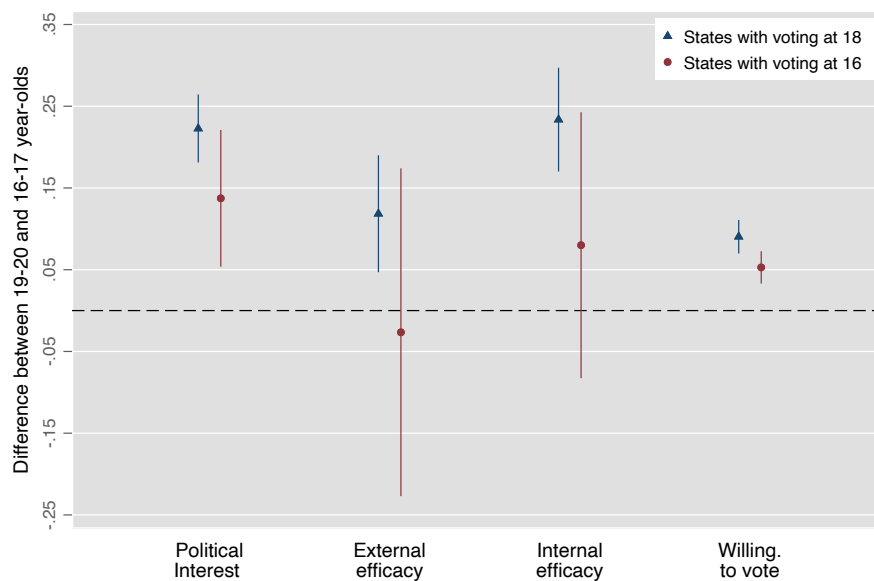
Table 3.2 Effect of enfranchisement on political maturity (control age-group 19-20 years)

	(1)	(2)	(3)	(4)	(5)
Outcome	Pol. interest	Ext. efficacy	Int. efficacy	Will. to vote	Att. consist.
Dataset	(JS/AIDA)	(JS)	(JS)	(JS)	(JS/AIDA)
Treated state (voting at 16)	-0.292*** (0.021)	-0.185** (0.064)	-0.123*** (0.039)	0.286*** (0.008)	0.074*** (0.008)
Treated group (16-17 years)	-0.228*** (0.020)	-0.120*** (0.033)	-0.234*** (0.030)	-0.091*** (0.009)	-0.014 (0.013)
Treated state * treated group	0.094** (0.037)	0.142 (0.108)	0.159** (0.066)	0.040** (0.014)	-0.012 (0.016)
Observations	11,815	6,976	6,976	6,990	9,400
Reduction diff. btw. groups	41%	-	68%	44%	-

Note: OLS with state-, year-, and state-year fixed effects. Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated group (1 if respondent is aged 16-17, 0 otherwise). 19-20 year-olds are the control age group. The last row displays the percentage reduction in the difference between underage and adult youth attributable to voting at 16, using the estimated difference in control states as baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results show that voting at 16 can equalise prior differences in political maturity between underage and adult youth. The negative coefficient on Treated group (16-17 years) in column 1 indicates that disenfranchised underage youth are, on average, less politically interested than adult youth. However, the positive coefficients on the interaction suggest that this difference is reduced significantly (by 41%) when 16-17 year-olds are enfranchised. I find even stronger equalising effects for internal efficacy (column 3) and willingness to vote (column 4). For internal efficacy, prior differences between underage and adult youth disappear completely when voting at 16 is implemented (see Figure 3.3). Voting at 16 also appears to equalise prior differences in external efficacy between age groups (column 2), but the effect is not significant at conventional levels. Finally, I find no significant difference in attitudinal consistency between disenfranchised 16-17 year-olds and enfranchised adult youth, and voting at 16 does not appear to change this (column 5).

Fig. 3.3 Voting at 16 reduces differences in political maturity between underage and adult youth



Note: Predicted values from OLS with cluster-robust standard errors. Blue triangles refer to the estimated difference in outcomes between 19-20 year-olds and 16-17 year-olds in states where the latter are disenfranchised. Red dots refer to the estimated difference in states with voting at 16. Vertical lines are 95% confidence intervals. Attitudinal consistency is omitted given that there are no significant differences between age groups at baseline.

A variety of robustness checks support the main findings. First, I use data on political interest from a different data source - the SOEP - to replicate the main analysis. The results

are very similar, with a reduction of prior differences between age groups of around one third (Table B18 in Appendix F). Second, the results hold when using logistic regression to predict binary and ordinal outcomes, and when using 21-22 year-olds as the control age group (Tables B3-B6 in Appendix E). Third, the results are robust to including several time-varying state-level and individual-level controls (Tables B8-B11 in Appendix F). Fourth, I use an event study approach to assess parallel trends. The non-significant placebo treatment effects for all survey years leading up to reform support the identification strategy (Figure B5 in Appendix F). Fifth, the results are robust to including state-specific linear time trends (Table B13 in Appendix F). Sixth, I conduct placebo tests by comparing age groups that should theoretically remain unaffected by the voting age reform. The null findings support the identification strategy (Table B14 in Appendix F). Seventh, I show that the sample composition remains unaffected by reform – a crucial assumption when using repeated cross-sections (Table B15 in Appendix F). Finally, I employ a wild bootstrap procedure to obtain more accurate p-values. The bootstrap results support the main findings, although the p-values tend to be more conservative (Table B16 in Appendix F).

3.6 Mechanisms

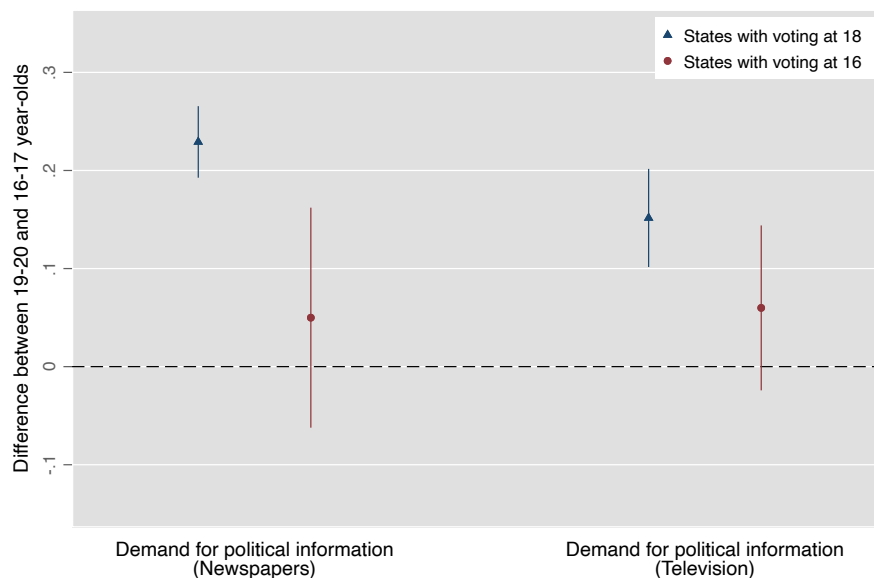
Why do we observe a reduction in political maturity differences between underage and adult youth when voting at 16 is introduced? There are two complementary channels through which this effect could operate. On the demand-side, newly enfranchised youth may actively seek out political information in order to inform (or justify) their vote. On the supply-side, political parties, civic groups, and schools may target newly enfranchised 16-17 year-olds with political education initiatives – as was the case during the voting age reforms in Germany (Vehrkamp et al., 2015), Austria (Zeglovits and Zandonella, 2013) and Scotland (Eichhorn, 2017). While the equalising effect of the demand-side mechanism should in theory affect every cohort of enfranchised 16-17 year-olds, not just the first one, the supply-side effect could be relatively short-lived if, for example, political education initiatives only target the first cohort of newly enfranchised voters.

Table B7 in Appendix E shows that prior differences between underage and adult youth in demand for political information from newspapers and television disappear when voting at 16 is implemented (see Figure 3.4 for a summary).¹⁸ This indicates that the equalising effect of voting at 16 on young people's political maturity may operate through increasing

¹⁸Similarly, Leininger and Faas (2020) find that enfranchisement is associated with an increase in voting advice application usage amongst 16-year-olds in Schleswig-Holstein.

demand for political information amongst the newly enfranchised. However, I cannot rule out that political maturity is the mediator and demand for political information is the outcome. Furthermore, there is evidence that the demand effect may be externally induced by political education initiatives implemented alongside the reforms, rather than by enfranchisement alone. Using an event study approach, I show that the equalising effect of voting at 16 on demand for political information is largely confined to the first cohort of newly enfranchised voters, which points in the direction of the supply-side mechanism (see Figure B6 in Appendix F). Finally, I find some evidence that voting at 16 reduces prior differences between underage and adult youth in terms of how frequently they discuss politics at school or university, which also points towards the supply-side mechanism (Table B7 in Appendix E).¹⁹

Fig. 3.4 Voting at 16 reduces differences in demand for political information between underage and adult youth



Note: Predicted values from OLS with state- and survey-year fixed effects. Blue triangles refer to the estimated difference in outcomes between 19-20 year-olds and 16-17 year-olds in states without voting at 16. Red dots refer to the estimated difference in states with voting at 16. The outcomes are reported frequency of using newspapers/television as a source of information about politics, ranging from 'never' (1) to 'very often' (4). Vertical lines are 95% confidence intervals. Data: Waves 2-3.

¹⁹I cannot disentangle demand- and supply-side determinants of political discussions at school. However, the important influence of schooling on young people's political attitudes is well documented (Dassonneville et al., 2012; Neundorff et al., 2016; Quintelier, 2010; Torney-Purta, 2002).

3.7 Discussion

This paper leverages a quasi-experiment in Germany to provide causal evidence that voting at 16 can equalise political maturity differences between underage and adult youth. The findings substantiate previous correlational evidence from Scotland and Austria (Eichhorn, 2017; Wagner et al., 2012; Zeglovits and Zandonella, 2013) and contribute to the growing (quasi-) experimental literature on transformative voting (Braconnier et al., 2017; Holbein et al., 2020; Shineman, 2016). Evidence on potential mechanisms suggests that the equalising effect of voting at 16 may operate through increasing demand for political information amongst the newly enfranchised. However, the relatively short-lived nature of this demand effect indicates that it may be induced by education initiatives implemented alongside the reform, rather than by enfranchisement alone.

The findings have important implications for contemporary and historical franchise debates, as they provide a rebuttal to the critics of franchise extension – albeit with a caveat. On the one hand, they cast doubts on the implicit assumption behind the critics’ argument, which is that citizens acquire political maturity independently of whether or not they can vote (Chan and Clayton, 2006; Keyssar, 2009; McAllister, 2014). On the other hand, the results also suggest that enfranchisement alone is unlikely to be sufficient for equalising maturity differences between underage and adult youth, and that political education initiatives may be crucial for ensuring that 16-year-olds can rise to the occasion. Further research will be necessary to establish precisely under which conditions enfranchisement can improve citizens’ political maturity.

3.8 Appendix

A Timeline

Table B1 Timeline of voting age reform in Germany

State	Date of first vote at 16
<i>State elections</i>	
Bremen	22 May 2011
Brandenburg	14 Sep 2014
Hamburg	15 Feb 2015
Schleswig-Holstein	07 May 2017
<i>Municipal elections</i>	
Niedersachsen	15 Sep 1996
Schleswig-Holstein	22 Mar 1998
Sachsen-Anhalt	13 Jun 1999
Mecklenburg-Vorpommern	13 Jun 1999
Nordrhein-Westfalen	12 Sep 1999
Berlin (Bezirksversammlung)	17 Sep 2006
Bremen (Beirat)	13 May 2007
Brandenburg	25 May 2014
Hamburg (Bezirksversammlung)	25 May 2014
Baden-Württemberg	25 May 2014
Thüringen	26 May 2019

Note: In Bremen, state and municipal elections take place at the same time using the same ballot paper, except for the local legislature of Bremerhaven, which is elected with a separate ballot paper on the same day as the state elections. Hessen introduced voting at 16 for state elections in 1998 but rescinded the law in 1999.

B Measuring political maturity

There is no consensus in the literature on how to best measure citizens' political maturity and previous studies on voting at 16 have used several different measures to approximate the concept. Here, I provide a brief overview of how previous studies have measured political maturity. The list of studies on voting at 16 included is not meant to be exhaustive and is based on an informal rather than a systematic literature review.

Interest in politics is perhaps one of the most frequently used proxy measure of political maturity. Political interest is typically operationalised using a single survey item, which asks respondents to indicate on a 4- or 5-point response scale how interested in politics they are

generally speaking (Aichholzer and Kritzinger, 2020; Bergh, 2013; Chan and Clayton, 2006; Eichhorn, 2014; Hart and Atkins, 2011; Leininger and Faas, 2020; Mahéo and Bélanger, 2020; McAllister, 2014; Stiers, Hooghe and Dassonneville, 2020; Wagner et al., 2012; Zeglovits and Zandonella, 2013).

Internal efficacy refers to the belief that one can understand politics and therefore participate in politics. This variable is either operationalised using a single survey item or a battery of survey items, where a latent construct (internal efficacy) is extracted using factor analysis or principal component analysis (Aichholzer and Kritzinger, 2020; Bergh, 2013; Eichhorn, 2014; Hart and Atkins, 2011; Mahéo and Bélanger, 2020; Stiers, Hooghe and Dassonneville, 2020).

External efficacy refers to the belief that government will respond to one's demands. As internal efficacy, this variable is either operationalised using a single survey item or several survey items, where a latent construct (external efficacy) is extracted using factor analysis or principal component analysis (Aichholzer and Kritzinger, 2020; Bergh, 2013; Eichhorn, 2014; Hart and Atkins, 2011; Stiers, Hooghe and Dassonneville, 2020).

Willingness to vote captures self-reported turnout intention and is often operationalised by asking respondents how likely they would be to vote in an upcoming election or if an election took place next week (Bronner and Ifkovits, 2019; Eichhorn, 2014, 2017; Mahéo and Bélanger, 2020; McAllister, 2014; Wagner et al., 2012).

Non-electoral participation refers to a range of different proxy measures aimed at capturing citizens' civic engagement outside the voting booth. The measures range from reported participation in demonstrations, petitions, boycotts, or writing to a member of parliament (Eichhorn, 2017; Wagner et al., 2012), reported participation in referendums and participatory budget consultations (Mahéo and Bélanger, 2020), to reported participation in community service (Hart and Atkins, 2011) or high-school activities (Zeglovits and Zandonella, 2013).

Attitudinal consistency captures the extent to which respondents hold internally consistent attitudes in one or several political domains (issue areas). For example, a respondent can be considered to hold inconsistent attitudes towards gender equality if she agrees that men should be the main breadwinners and, at the same time, also agrees that there should be many more women in political and public leadership roles. Previous studies on voting at 16 operationalise attitudinal consistency with reference to several issue areas, including trust in government, women's employment, immigration, and climate change (Bergh, 2013; Chan and Clayton, 2006; Mahéo and Bélanger, 2020).

Political knowledge is a widely used proxy measure of political maturity and captures respondents' objective knowledge about political facts. Previous studies operationalise political knowledge by aggregating respondents' correct answers to a battery of factual knowledge questions (e.g. Margaret Thatcher was a Conservative prime minister. Correct or incorrect?) (Chan and Clayton, 2006; Leininger and Faas, 2020; Mahéo and Bélanger, 2020; McAllister and Studlar, 2002; Stiers, Hooghe and Dassonneville, 2020), using correct party placement on a left-right ideological scale (Wagner et al., 2012; Zeglovits and Zandonella, 2013), or using high-school grades in social studies (Rosenqvist, 2017).

Congruence of political preferences and vote choice aims to capture how close a respondents' reported vote choice is to her ideological self-placement on a left-right scale. This is either operationalised as the absolute distance between ideological self-placement and preferred party's placement (by experts) on 10-point scale (Wagner et al., 2012), or as a binary indicator of "correct voting" if reported vote choice is the one that is closest to ideological self-placement (Aichholzer and Kritzing, 2020; Stiers, Hooghe and Goubin, 2020).

C Variable coding

Interest in politics

Interest in politics is measured by the question 'How strong is your interest in politics?' with five possible answers ranging from 'very strong' to 'not at all'. The official English translation of this survey item is awkward. In German, the question is 'Wie stark interessieren Sie sich für Politik?' with answers ranging from 'überhaupt nicht' (1) to 'sehr stark' (5). The survey item is available in both JS and AIDA (waves 1-5).

External and internal efficacy

To measure external and internal efficacy, two continuous latent variables are extracted from respondents' answers to a battery of six survey questions on political efficacy. External efficacy refers to the belief that government will respond to one's demands, whereas internal efficacy refers to the belief that one can understand politics and therefore participate in politics (Balch, 1974). The six survey items capturing respondents' political efficacy were taken from a larger battery included in the American National Election Studies (ANES) and are available only in the JS (waves 1-3). Respondents were asked to agree or disagree (on a 6-point scale) with each of the following statements: (a) I don't think that politicians

worry too much about what people like me think; (b) I understand a lot about politics; (c) People like me have no influence one way or the other on what the government does; (d) Politicians are only interested in getting elected and not in what the voters really want; (e) Sometimes I think politics is so complicated that a normal person can hardly understand it; (f) In our society there are only a few powerful people; everyone else has no influence on what the government does. Note that item (b) is positively phrased, whilst all other items are negatively phrased. All efficacy items are re-coded so that 1 = lowest efficacy and 6 = highest efficacy.

To extract continuous latent variables from this battery of efficacy items, I use principal component analysis (PCA) on the two sub-sample of respondents that are subsequently used in the regression analysis (16-17 and 19-20 year-olds as well as 16-17 and 21-22 year-olds). The PCA results presented in Table B2 indicate that two principal components (latent variables) capture 62% of the variance from the six efficacy items. Whilst the first two components have an eigenvalue that is larger than 1, the third component has an eigenvalue of 0.7 (so its variance is smaller than the variance of the original standardised item). This suggest that only the first two components should be retained for analysis. The component loadings (eigenvectors) reveal that items (a), (c), (d) and (f) contribute mostly to the first component, whilst items (b) and (e) contribute to the second component. This suggests that the first component captures respondents' external efficacy and the second component captures respondents' internal efficacy. For subsequent regression analysis, the principle component scores are standardised to have a mean of 0 and a standard deviation of 1.

Table B2 Principal component analysis of six political efficacy items

Sample: 16-17 and 19-20 years (N=6,976)						
Principal components (eigenvectors)						
	(1)	(2)	(3)	(4)	(5)	(6)
Items	(a)	.4563474	-.1774939	.3027249	.6971309	.1027648
	(b)	.1371869	.7894974	.5661603	-.1415197	.1299322
	(c)	.463957	-.124789	.1839999	-.2490729	-.8169026
	(d)	.4840671	-.1927633	.0398601	.014327	.3809809
	(e)	.3172334	.5323316	-.7165133	.2829612	-.1452329
	(f)	.4719245	-.0953037	-.1974436	-.5930186	.372811
Sample: 16-17 and 21-22 years (N=6,782)						
Principal components (eigenvectors)						
	(1)	(2)	(3)	(4)	(5)	(6)
Items	(a)	.4557548	-.1513113	.3713816	-.6608721	.2110673
	(b)	.1272811	.7852305	.5319996	.2500836	.1469863
	(c)	.4677762	-.1040588	.1921949	.1720259	-.8385748
	(d)	.4840156	-.2169304	.0605567	.0803673	.3499696
	(e)	.3202639	.5396529	-.6684594	-.3704737	-.1147149
	(f)	.4695022	-.1067973	-.3026685	.572192	.3082204

Note: In the first panel: Component (1) eigenvalue: 2.60302; Component (2) eigenvalue: 1.1601 Cumulative explained variance of (1) and (2): 0.6225. In the second panel: Component (1) eigenvalue: 2.56952; Component (2) eigenvalue: 1.13188. Cumulative explained variance of (1) and (2): 0.6216. The table presents principal components (eigenvectors) from a PCA of the correlation matrix of the six efficacy items, using two sub-samples from the JS 1992-2003 data. The first sample contains 16-17 and 19-20 year-olds and the second sample contains 16-17 and 21-22 year-olds. The individual components are orthogonal to each other. Grey shading is used to highlight how the first two components load on the six efficacy items.

Willingness to vote

Willingness to vote is a binary indicator of whether respondents report that they would vote in a hypothetical election in order to exert political influence. The relevant survey prompt reads as follows: Let's assume you would like to exert political influence or make your viewpoint known on a subject that is important to you. Which of the following possibilities would you consider and which would you not? Relevant response item: (a) Vote at elections. Item is only available in the JS data (waves 1-3).

Attitudinal consistency

Attitudinal consistency is measured using a binary variable, where 1 identifies respondents with consistent attitudes and 0 identifies respondents with inconsistent attitudes. Respondents

with consistent attitudes agree or disagree with all of three statements about the role of women in society, whereas inconsistent respondents agree with some of the statements and disagree with others. The survey prompt reads as follows: The following question deals with the everyday situation of men and women. To what extent do you believe in these statements? Number one (1) means you don't agree at all and a number six (6) means you completely agree. Use the numbers in between to approximate the level to which you agree.

1. Even if a woman works the man should be the main earner and the woman should be responsible for the household.
2. There should be many more women in political and public leadership roles.
3. If a family has children, the man should work and the woman should stay at home and care for the children.

Responses to item 2 are reverse-coded so that for all three items, higher scores mean stronger anti-gender equality attitudes. PCA on the complete sample (16-29 year-olds, waves 1-4) suggests the three items are good at capturing a single latent dimension of respondents' general attitude towards gender equality, with weak evidence for a second dimension capturing attitude towards women in public positions. The first component (general attitude towards gender equality) has an eigenvalue of 1.76 and explains 59% of the variance from the three survey items. The second component (attitude towards women in public positions) has a much smaller eigenvalue of 0.84 and only explains 28% of the variance from the three survey items. The Cronbach's alpha of 0.64 is adequate in light of only three items and suggests that the items have relatively high internal consistency. Responses above 3 are treated as agreement with the statement and responses below or equal to 3 are treated as disagreement. Items are available in JS 1992-2003 and AIDA 2009 (waves 1-4).

Demand for political information from newspapers and television

Demand for political information from newspapers and television are measured by the question 'How often do you inform yourself about political topics from the following sources?' with 'newspapers/magazines' and 'television' as possible sources, and four possible answer options for all sources ranging from 1 (never) to 4 (very often). Other possible sources (not used in the analysis) are: books, radio, internet, conversations/discussions, and attending a political event. The two items for newspaper and television consumption are available in the JS data from 1997 and 2003 (waves 2-3).

Frequency of political discussions at school/university

Frequency of political discussion at school (or university) is measured on a four-point scale running from 1 (never) to 4 (very often) based on respondents answer to the following prompt: 'Please think again about the people with whom you discuss political questions. How often do you talk with the people, whom I'm about to mention, about politics? How often do you talk... A) with your mother? B) with your father? C) with your partner or spouse? D) with friends or acquaintances? E) with schoolmates or fellow university students? F) with colleagues? G) with your siblings?' Answers to option E) are used to measure the frequency of political discussions at school (or university). The political discussion item is available in the JS data (waves 1-3).

Controls

Data on time-varying state-level covariates come from various sources. Data on state-level unemployment rates (years available: 1994-2018) and public education spending (years available: 1995-2018) come from the German Federal Statistical Office.²⁰ State-level population data (years available: 1990-2015) come from the Quality of Government EU Regional Database.²¹ Data on state-level voter turnout and vote share of the centre-right CDU in the most recent general election (years available: 1990-2009) come from the European Election and Referendum Database.²² Data on individual-level controls come from the JS and AIDA surveys. Respondents' gender is measured using a binary indicator equal to 1 for female respondents and 0 for male respondents. The item is available in all survey waves. The binary indicator of whether a respondent still lives with her parents at the time of the survey is coded from two distinct items in the JS and AIDA surveys. In the JS, respondents were asked: Do you live in your parent's household all the time or most of the time? Birth parents, adoptive parents, stepparents, foster parents and single parents are meant here!, with the answer options being yes and no. In the AIDA survey, respondents were instead asked whether they have moved out of their parent's house (Sind Sie schon einmal aus der elterlichen Wohnung bzw. Ihrem Elternhaus ausgezogen?), with the answer options being yes and no.

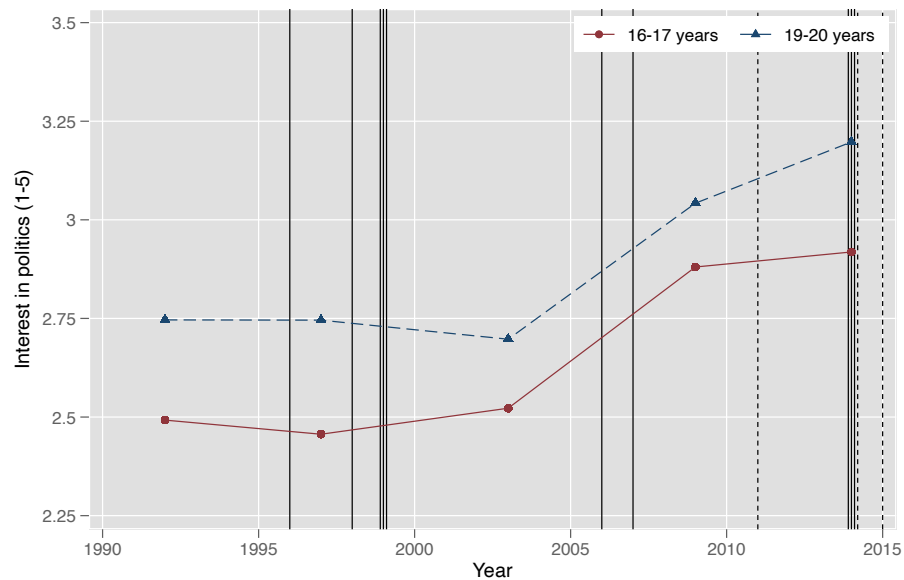
²⁰ Available at: <https://www-genesis.destatis.de/genesis/online/data>

²¹ QoG EU Regional Long Data (version: Sep 2016). Available at: <https://qog.pol.gu.se/data/datadownloads/qogeuregionaldata>

²² Available at: https://nsd.no/european_election_database

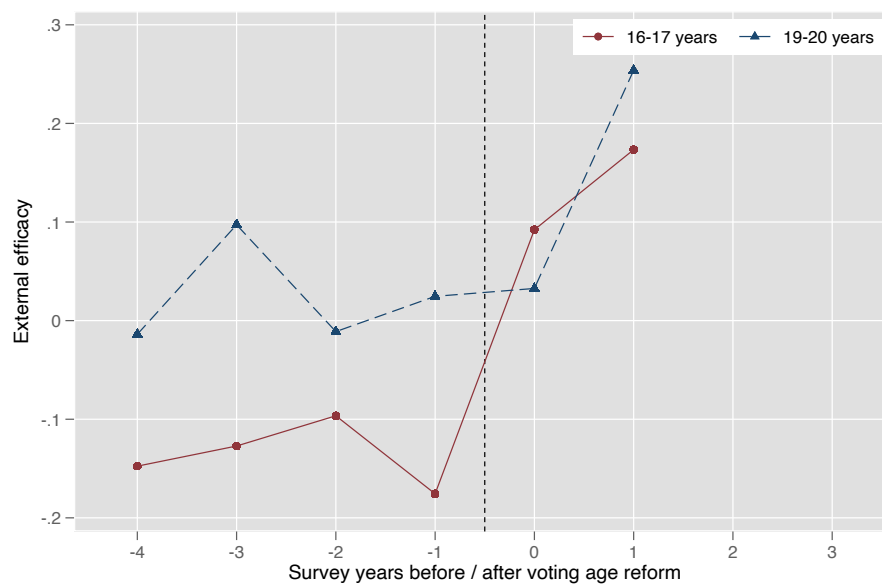
D Descriptive results

Fig. B1 Interest in politics by age group - non-switching control states



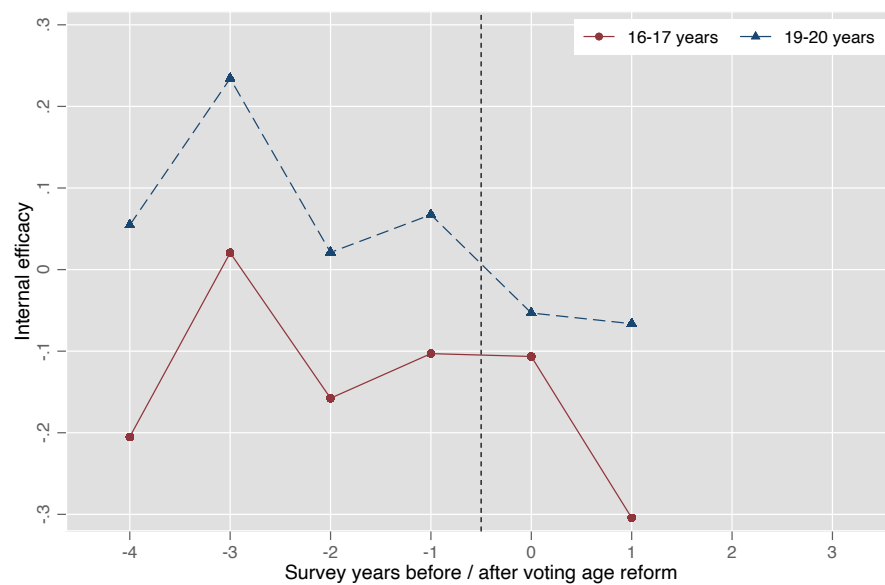
Note: The plot shows the average interest in politics amongst 16-17 year-olds (red dots) and 19-20 year-olds (blue triangles) for all available survey years (1992-2014). The sample is restricted to states that did not switch to voting at 16. Solid vertical lines represent the introduction of voting at 16 in municipal elections. Dashed vertical lines represent the introduction of voting at 16 in state elections.

Fig. B2 External efficacy by age group – switching states



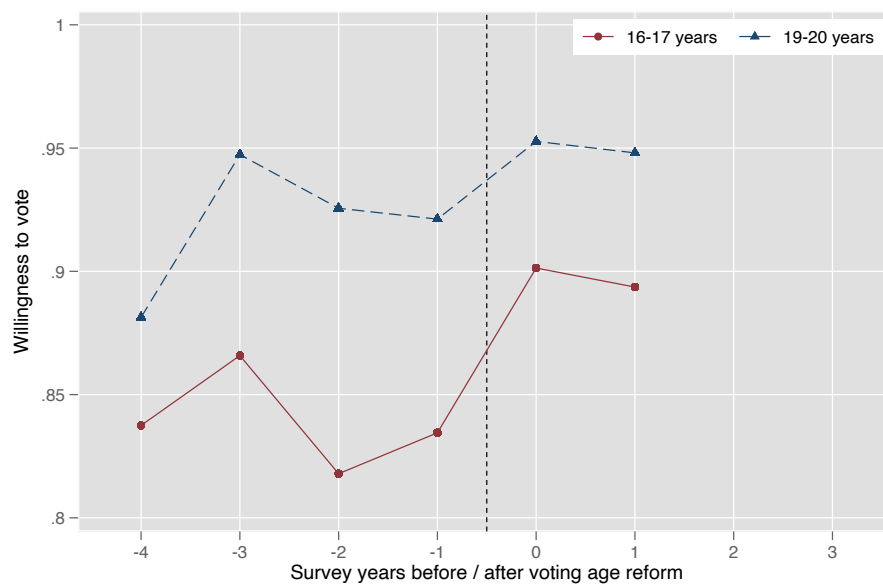
Note: The plot shows the average external efficacy amongst 16-17 year-olds (red dots) and 19-20 year-olds (blue triangles) for all survey years before and after the voting age reform (vertical dashed line). The sample is restricted to states that switched to voting at 16 and the data are normalised so that year zero refers to the first survey year after the voting age reform.

Fig. B3 Internal efficacy by age group – switching states



Note: The plot shows the average internal efficacy amongst 16-17 year-olds (red dots) and 19-20 year-olds (blue triangles) for all survey years before and after the voting age reform (vertical dashed line). The sample is restricted to states that switched to voting at 16 and the data are normalised so that year zero refers to the first survey year after the voting age reform.

Fig. B4 Willingness to vote by age group – switching states



Note: The plot shows the average willingness to vote amongst 16-17 year-olds (red dots) and 19-20 year-olds (blue triangles) for all survey years before and after the voting age reform (vertical dashed line). The sample is restricted to states that switched to voting at 16 and the data are normalised so that year zero refers to the first survey year after the voting age reform.

E Regression results

Interest in politics

Table B3 presents results for regression models predicting interest in politics. Models 1-2 present results from linear fixed effects regression models, using 19-20 year-olds and 21-22 year-olds as control groups respectively. The results indicate that disenfranchised underage youth are on average less interested in politics than (enfranchised) adult youth, but that this difference is significantly reduced – by around one third – when 16-17 year-olds are enfranchised. Models 3-4 in turn present results from ordered logistic regression models predicting respondents' interest in politics. The results point in the same direction as the findings from the linear models – enfranchising 16-17 year-olds significantly reduces the difference in interest in politics between underage and adult youth. Finally, Models 5-6 present results from binary logistic regression models (using a binary version of the outcome variable). The results from the binary logistic models are consistent with findings from the linear models and the ordered logistic models.

Table B3 Effect of enfranchisement on interest in politics

Control age group	19-20	21-22	19-20	21-22	19-20	21-22
Model	OLS	OLS	Ord. Log.	Ord. Log.	Bin. Log.	Bin. Log.
Treated state (voting at 16)	-0.292*** (0.021)	-0.712*** (0.024)	0.545*** (0.020)	0.288*** (0.011)	0.534*** (0.029)	0.237*** (0.012)
Treated group (16-17 years)	-0.228*** (0.020)	-0.295*** (0.031)	0.642*** (0.025)	0.571*** (0.030)	0.637*** (0.030)	0.568*** (0.040)
Treated state * treated group	0.094** (0.037)	0.122** (0.049)	1.229*** (0.082)	1.283*** (0.108)	1.331*** (0.138)	1.404*** (0.143)
Constant	3.053*** (0.012)	3.097*** (0.016)	-	-	0.533*** (0.015)	0.507*** (0.016)
Observations	11,815	11,339	11,815	11,339	11,815	11,339

Note: Coefficients (or odds ratios in the case of the logistic models) are shown with cluster-robust standard errors in parentheses. The outcome variable in Models 1-4 is respondents' interest in politics, ranging from 0 (not at all) to 5 (very strong). The outcome variable in Models 5-6 is respondents' interest in politics measured as a binary indicator (1 if respondent is "strongly" or "very strongly" interested in politics, 0 otherwise). The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Intercepts (cut-points) are omitted from the ordered logistic regression output to save space. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. Data: Waves 1-5. *** p<0.01, ** p<0.05, * p<0.1

External and internal efficacy

Table B4 presents results from linear fixed effects regression models predicting respondents' external efficacy (the belief that government will respond to one's demands) and internal efficacy (the belief that one can understand politics and therefore participate in politics).

Table B4 Effect of enfranchisement on external and internal efficacy

Control age group	19-20	21-22	19-20	21-22
Model	OLS	OLS	OLS	OLS
Treated state (voting at 16)	-0.185** (0.064)	-0.295** (0.103)	-0.123*** (0.039)	0.036 (0.068)
Treated group (16-17 years)	-0.120*** (0.033)	-0.153*** (0.018)	-0.234*** (0.030)	-0.329*** (0.041)
Treated state * treated group	0.142 (0.108)	0.137 (0.144)	0.159** (0.066)	0.126 (0.100)
Constant	0.071*** (0.021)	0.241*** (0.009)	0.234*** (0.019)	0.234*** (0.021)
Observations	6,976	6,782	6,976	6,782

Note: Coefficients are shown with cluster-robust standard errors in parentheses. The outcome variable in Models 1-2 is a latent external efficacy score, standardised to have a mean of 0 and a standard deviation of 1. The outcome variable in Models 3-4 is a latent internal efficacy score, standardised to have a mean of 0 and a standard deviation of 1. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. Data: Waves 1-3. *** p<0.01, ** p<0.05, * p<0.1

Willingness to vote

Table B5 presents results for regression models predicting respondents' willingness to vote in a hypothetical election in order to exert political influence. Models 1-2 present results from linear probability models (OLS). Models 3-4 present results from binary logistic regression models. The results point in the same direction as those from the linear probability models presented in the main text – enfranchising 16-17 year-olds appears to reduce the difference in willingness to vote between underage and adult youth. Note, however, that the results are only statistically significant at conventional levels when 21-22 year-olds are used as the within-state control group.

Table B5 Effect of enfranchisement on willingness to vote

Control age group	19-20	21-22	19-20	21-22
Model	OLS	OLS	Bin. Log.	Bin.Log
Treated state (voting at 16)	0.286*** (0.008)	0.178*** (0.010)	17.752*** (2.307)	6.805*** (1.132)
Treated group (16-17 years)	-0.091*** (0.009)	-0.107*** (0.010)	0.414*** (0.031)	0.324*** (0.038)
Treated state * treated group	0.040** (0.014)	0.064*** (0.015)	1.100 (0.199)	1.703** (0.368)
Constant	0.812*** (0.006)	0.868*** (0.005)	5.606*** (0.308)	8.600*** (0.701)
Observations	6,990	6,764	6,978	6,764

Note: Coefficients (or odds ratios in the case of logistic models) are shown with cluster-robust standard errors in parentheses. The outcome variable is a binary variable (1 if respondent is willing to vote, 0 otherwise). The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Data: Waves 1-3. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1

Attitudinal consistency

Table B6 presents results from regression models predicting respondents' attitudinal consistency. Models 1-2 present results from linear probability models (OLS). The non-significant coefficients on the age group indicator (16-17 years) suggest that there are no differences in attitudinal consistency between disenfranchised underage and enfranchised adult youth. Furthermore, the non-significant coefficients on the interaction term indicate (when the default comparison group of 19-20 year-olds is used) that the introduction of voting at 16 does little to change this. Results from binary logistic regression models (Models 3-4) are consistent with results from the linear probability models.

Table B6 Effect of enfranchisement on attitudinal consistency

Control age group	19-20	21-22	19-20	21-22
Model	OLS	OLS	Bin. Log.	Bin.Log
Treated state (voting at 16)	0.074*** (0.008)	0.232*** (0.007)	1.371*** (0.043)	2.597*** (0.070)
Treated age group (16-17 years)	-0.014 (0.013)	-0.015 (0.011)	0.945 (0.048)	0.942 (0.042)
Treated state * treated age group	-0.012 (0.016)	-0.027** (0.011)	0.951 (0.062)	0.897** (0.042)
Constant	0.542*** (0.008)	0.452*** (0.006)	1.184*** (0.038)	0.825*** (0.019)
Observations	9,400	8,878	9,400	8,878

Note: Coefficients (or odds ratios in the case of logistic models) are shown with cluster-robust standard errors in parentheses. The outcome variable is a binary measure of attitudinal consistency (1 identifies respondents with consistent attitudes and 0 identifies respondents with inconsistent attitudes). The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Data: Waves 1-4. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs.*** p<0.01, ** p<0.05, * p<0.1

Demand for political information

Table B7 presents results from regression models predicting several measures of demand for political information. Models 1-2 present results for respondents' reported use of newspapers as a source of political information. Models 3-4 present results for respondents' reported use of television as a source of political information. Finally, Models 5-6 presents results for reported frequency of political discussions with their schoolmates or fellow university students. For the political discussions measure the interaction effect does not reach conventional significance thresholds (regardless of the comparison age group used).

Table B7 Effect of enfranchisement on demand for political information

Outcome	Newspapers		Television		Pol. discussions	
Control age group	19-20	21-22	19-20	21-22	19-20	21-22
Model	OLS	OLS	OLS	OLS	OLS	OLS
Treated state (voting at 16)	-0.129*** (0.031)	0.113*** (0.013)	-0.225*** (0.019)	0.009 (0.030)	0.076*** (0.025)	0.031 (0.051)
Treated group (16-17 years)	-0.222*** (0.017)	-0.266*** (0.021)	-0.146*** (0.025)	-0.121*** (0.025)	-0.048 (0.029)	-0.108*** (0.036)
Treated state * treated group	0.180*** (0.054)	0.144*** (0.023)	0.091** (0.035)	0.018 (0.046)	0.063 (0.040)	0.096 (0.065)
Constant	2.685*** (0.011)	2.635*** (0.014)	2.986*** (0.016)	2.883*** (0.016)	2.350*** (0.019)	2.404*** (0.021)
Observations	5,219	4,880	5,220	4,883	5,932	5,413

Note: In Models 1-2 the outcome variable measures respondents' reported frequency of using newspapers as a source of information about politics, ranging from 'never' (1) to 'very often' (4). In Models 3-4 the outcome variable measures respondents' reported frequency of using television as a source of information about politics, ranging from 'never' (1) to 'very often' (4). In Models 5-6 the outcome variable measures respondents' reported frequency of discussing political issues with their schoolmates (or fellow university students), with answer options ranging from 'never' (1) to 'very often' (4). The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Data: Waves 2-3. *** p<0.01, ** p<0.05, * p<0.1

F Robustness checks

Adding controls

To assess the robustness of the results, I include several time-varying state-level controls as well as individual-level controls in the baseline regression model. Individual-level controls are not necessary for causal identification but can increase the precision of the estimates (Angrist and Pischke, 2009). The set of time-varying state-level controls includes economic shocks, captured by the local unemployment rate, demographic shocks, captured by the local (log) population, and political preference shocks, captured by the local vote share of the centre-right CDU in the last general election. Furthermore, I control for voter turnout in the last general election to capture the local level of political mobilisation, and public education spending per capita as a proxy for state-level shocks to the education system. Individual-level controls are respondents' gender as well as an indicator of whether they still live with their parents at the time of the survey. Most of the results are robust to the inclusion of time-varying state-level and individual-level controls. Note, however, that with controls, the results for internal efficacy fall just short of the conventional significance level.

Table B8 Effect of enfranchisement on interest in politics (with controls)

Control age group	19-20 years		21-22 years	
Model	(1)	(2)	(3)	(4)
Treated state (voting at 16)	-0.084 (0.054)	-0.083 (0.053)	-0.066* (0.037)	-0.056 (0.039)
Treated group (16-17 years)	-0.235*** (0.021)	-0.227*** (0.024)	-0.296*** (0.034)	-0.254*** (0.030)
Treated state * treated group	0.099** (0.038)	0.086** (0.038)	0.124** (0.046)	0.106** (0.045)
Constant	14.883* (8.106)	12.583 (7.756)	18.588* (9.258)	17.398* (8.894)
Time-varying state-level controls	✓	✓	✓	✓
Individual-level controls		✓		✓
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Cluster-robust SEs	✓	✓	✓	✓
Observations	10,026	10,013	9,402	9,386

Note: Note: OLS models with state- and survey-year fixed effects. Models 1 and 3 control for time-varying state-level covariates (unemployment rate, log population, CDU vote share, turnout, public education spending per capita). Models 2 and 4 in addition control for individual-level covariates (gender, lives with parents). Coefficients are shown with robust standard errors in parentheses. The outcome variable is respondents' interest in politics, ranging from 0 (not at all) to 5 (very strong). The key independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Models 1-2 use 19-20 year-olds as the control age group, and Models 3-4 use 21-22 year-olds as the control age group. Data: Waves 1-5. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B9 Effect of enfranchisement on external efficacy (with controls)

Control age group	19-20 years		21-22 years	
Model	(1)	(2)	(3)	(4)
Treated state (voting at 16)	-0.255** (0.095)	-0.260** (0.094)	-0.123 (0.129)	-0.129 (0.130)
Treated group (16-17 years)	-0.088** (0.034)	-0.092** (0.040)	-0.106*** (0.028)	-0.082** (0.033)
Treated state * treated group	0.115 (0.109)	0.116 (0.107)	0.085 (0.143)	0.084 (0.142)
Constant	43.386 (31.823)	42.584 (31.204)	21.138 (35.046)	23.563 (35.069)
Time-varying state-level controls	✓	✓	✓	✓
Individual-level controls		✓		✓
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Cluster-robust SEs	✓	✓	✓	✓
Observations	5,205	5,203	4,861	4,857

Note: Note: OLS models with state- and survey-year fixed effects. Models 1 and 3 control for time-varying state-level covariates (unemployment rate, log population, CDU vote share, turnout, public education spending per capita). Models 2 and 4 in addition control for individual-level covariates (gender, lives with parents). Coefficients are shown with robust standard errors in parentheses. The outcome variable is a latent external efficacy score, standardised to have a mean of 0 and a standard deviation of 1. The key independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Models 1-2 use 19-20 year-olds as the control age group, and Models 3-4 use 21-22 year-olds as the control age group. Data: Waves 1-3. *** p<0.01, ** p<0.05, * p<0.1

Table B10 Effect of enfranchisement on internal efficacy (with controls)

Control age group	19-20 years		21-22 years	
Model	(1)	(2)	(3)	(4)
Treated state (voting at 16)	-0.102 (0.078)	-0.124 (0.072)	-0.107 (0.115)	-0.115 (0.114)
Treated group (16-17 years)	-0.195*** (0.037)	-0.170*** (0.043)	-0.316*** (0.047)	-0.293*** (0.059)
Treated state * treated group	0.116 (0.072)	0.118 (0.069)	0.117 (0.100)	0.116 (0.099)
Constant	57.213*** (17.885)	57.167*** (17.775)	49.103** (18.741)	54.559** (19.481)
Time-varying state-level controls	✓	✓	✓	✓
Individual-level controls		✓		✓
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Cluster-robust SEs	✓	✓	✓	✓
Observations	5,205	5,203	4,861	4,857

Note: OLS models with state- and survey-year fixed effects. Models 1 and 3 control for time-varying state-level covariates (unemployment rate, log population, CDU vote share, turnout, public education spending per capita). Models 2 and 4 in addition control for individual-level covariates (gender, lives with parents). Coefficients are shown with robust standard errors in parentheses. The outcome variable is a latent internal efficacy score, standardised to have a mean of 0 and a standard deviation of 1. The key independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Models 1-2 use 19-20 year-olds as the control age group, and Models 3-4 use 21-22 year-olds as the control age group. Data: Waves 1-3. *** p<0.01, ** p<0.05, * p<0.1

Table B11 Effect of enfranchisement on willingness to vote (with controls)

Control age group	19-20 years		21-22 years	
Model	(1)	(2)	(3)	(4)
Treated state (voting at 16)	-0.040 (0.029)	-0.041 (0.030)	-0.048 (0.038)	-0.046 (0.038)
Treated group (16-17 years)	-0.074*** (0.008)	-0.080*** (0.008)	-0.082*** (0.008)	-0.087*** (0.006)
Treated state * treated group	0.021* (0.010)	0.021* (0.011)	0.041*** (0.013)	0.039** (0.013)
Constant	24.541** (9.025)	24.363** (9.085)	24.565** (9.959)	24.530** (9.912)
Time-varying state-level controls	✓	✓	✓	✓
Individual-level controls		✓		✓
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Cluster-robust SEs	✓	✓	✓	✓
Observations	5,222	5,220	4,877	4,872

Note: OLS models with state- and survey-year fixed effects. Models 1 and 3 control for time-varying state-level covariates (unemployment rate, log population, CDU vote share, turnout, public education spending per capita). Models 2 and 4 in addition control for individual-level covariates (gender, lives with parents). Coefficients are shown with robust standard errors in parentheses. The outcome variable is a binary variable (1 if respondent is willing to vote, 0 otherwise). The key independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Models 1-2 use 19-20 year-olds as the control age group, and Models 3-4 use 21-22 year-olds as the control age group. Data: Waves 1-3. *** p<0.01, ** p<0.05, * p<0.1

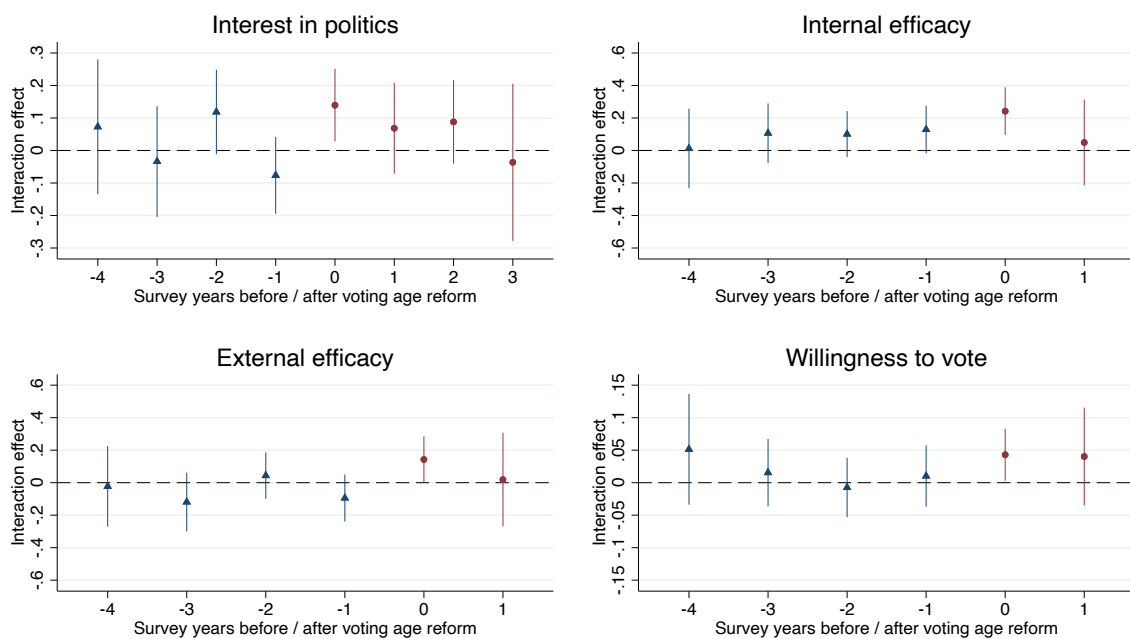
Dynamic treatment effects

To assess the plausibility of the parallel trends assumption and examine the persistence of treatment effects over time, I employ an event study framework. Specifically, I code a binary indicator that equals 1 for the first survey year after a state switched to voting at 16 (and 0 otherwise) and then include four leads and three lags of this switching indicator in the regression models, each one interacted with the treatment age group indicator.²³ Figure B5 presents the estimated interaction effects from event study models predicting several measures of political maturity (with 19-20 year-olds as the control age group). Figure B6 instead presents results from event study models predicting different measures of young people's demand for political information. I find no placebo treatment effects for the four survey years leading up to the voting age reform, which is evidence in support of the parallel trends assumption. The non-significant placebo effects for the survey years after the switch to voting at 16 may indicate that the initial equalising effect of the reform does not persist over time. However, these results should be interpreted with caution, given that the time series is quite short and the estimates are relatively imprecise.²⁴

²³In contrast to the treatment indicator used in the main analysis, the (real and placebo) switching indicators are equal to 1 only in the relevant survey year (see Autor 2003; Hainmueller and Hangartner 2019).

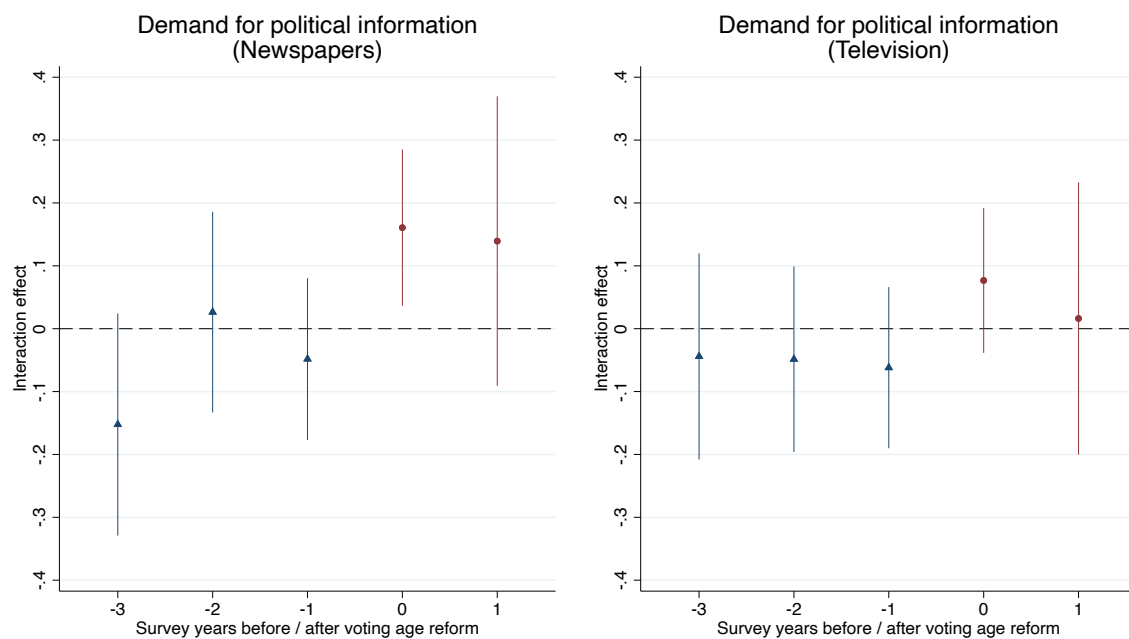
²⁴I also examine dynamic treatment effects using a semi-dynamic event study model, where all leads of the switching indicator are set to zero. This specification is more suitable for detecting dynamic treatment effects, as it does not suffer from the under-identification issues highlighted in Borusyak and Jaravel (2017). The results from the semi-dynamic model (available on request) also suggest that the initial equalising effect of the voting age reform does not persist over time.

Fig. B5 Dynamic effect of enfranchisement on difference in political maturity between underage and adult youth



Note: Point estimates show the marginal effect of switching to voting at 16 on outcomes of 16-17 year-olds relative to 19-20 year-olds for survey years before (blue triangles) and after (red dots) the voting age reform. Point estimates are from a linear fixed effects regression with four leads and three lags of the switching indicator, each interacted with the age group indicator. Switching indicators are equal to one only in the relevant survey year. Survey year 0 refers to the first survey year after voting at 16 was implemented. Vertical lines are 95% confidence intervals. 19-20 year-olds are the control age group. Data are from switching and non-switching states for all available survey waves.

Fig. B6 Dynamic effect of enfranchisement on difference in demand for political information between underage and adult youth



Note: Point estimates show the marginal effect of switching to voting at 16 on outcomes of 16-17 year-olds relative to 19-20 year-olds for survey years before (blue triangles) and after (red dots) the voting age reform. Point estimates are from a linear fixed effects regression with four leads and three lags of the switching indicator, each interacted with the age group indicator. Switching indicators are equal to one only in the relevant survey year. Survey year 0 refers to the first survey year after voting at 16 was implemented. Vertical lines are 95% confidence intervals. 19-20 year-olds are the control age group. Data are from switching and non-switching states for all available survey waves.

State-specific linear time trends

An alternative way to assess the plausibility of the parallel trends assumption is to relax the baseline model specification and estimate

$$Y_{igst} = \gamma_s + \gamma_s * t + \lambda_t + \delta_g + D_{st} + \beta(D_{st} * \delta_g) + (\gamma_s * \lambda_t) + X_{igst} + \varepsilon_{igst} \quad (3.3)$$

where the state fixed effect γ_s is interacted with a linear time index t (equal to 1 for survey wave 1, and 2 for survey wave 2, etc.). The inclusion of state-specific linear time trends ensures that unobserved state-specific differences that vary smoothly over time are purged from the estimate of β , so that only breaks in the local trends of political maturity differences between age groups that directly coincide with the voting age reform are captured by β . The results are robust to the inclusion state-specific linear time trends.

Table B12 Effect of enfranchisement on political maturity (state-specific time trends):
Control group (19-20 years)

Outcome	(1) Pol. interest	(2) Ext. efficacy	(3) Int. efficacy	(4) Will.to vote	(5) Att. consist.
Dataset	(JS/AIDA)	(JS)	(JS)	(JS)	(JS/AIDA)
Treated state (voting at 16)	-0.086* (0.048)	-0.210 (0.149)	-0.107 (0.135)	-0.008 (0.053)	-0.037 (0.037)
Treated group (16-17 years)	-0.222*** (0.020)	-0.118*** (0.034)	-0.232*** (0.030)	-0.091*** (0.009)	-0.012 (0.013)
Treated state * treated group	0.085** (0.038)	0.139 (0.108)	0.155** (0.066)	0.038** (0.014)	-0.017 (0.015)
Observations	11,815	6,976	6,976	6,990	9,400

Note: OLS models with state- and survey-year fixed effects and state-specific linear time trends. Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Data: Waves 1-5. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1

Table B13 Effect of enfranchisement on political maturity (state-specific time trends):
Control group (21-22 years)

Outcome	(1) Pol. interest	(2) Ext. efficacy	(3) Int. efficacy	(4) Will.to vote	(5) Att. consist.
Dataset	(JS/AIDA)	(JS)	(JS)	(JS)	(JS/AIDA)
Treated state (voting at 16)	-0.034 (0.044)	-0.084 (0.161)	-0.060 (0.188)	-0.044 (0.066)	0.005 (0.043)
Treated group (16-17 years)	-0.287*** (0.031)	-0.150*** (0.019)	-0.333*** (0.042)	-0.108*** (0.010)	-0.012 (0.010)
Treated state * treated group	0.114** (0.047)	0.133 (0.146)	0.133 (0.101)	0.066*** (0.016)	-0.033** (0.011)
Observations	11,339	6,782	6,782	6,764	8,878

Note: OLS models with state- and survey-year fixed effects and state-specific linear time trends. Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). Data: Waves 1-5. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1

Placebo tests

Another way to assess the robustness of the identification strategy is to conduct placebo tests. Using several measures of political maturity, I re-run the baseline model, but instead compare 18-19-year-olds to 21-22-year-olds. In theory, the introduction of voting at 16 should not affect the difference in political maturity between these two age groups, as both groups already have the right to vote before the reform. Table B14 presents results from linear fixed effects regression models predicting respondents' political maturity, where 18-19-year-olds are the placebo treatment group and 21-22-year-olds are the control group. Except for Model 4 (willingness to vote), the coefficients on the interaction terms are statistically insignificant, which is evidence in support of the identification strategy.

Table B14 Effect of enfranchisement on political maturity (placebo regressions)

Placebo treatment group	18-19 years				
Control age group	21-22 years				
	(1)	(2)	(3)	(4)	(5)
Outcome	Pol. interest	Ext. efficacy	Int. efficacy	Will.to vote	Att. consist.
Dataset	(JS/AIDA)	(JS)	(JS)	(JS)	(JS/AIDA)
Treated state (voting at 16)	-0.927*** (0.011)	-0.152** (0.055)	-0.259*** (0.036)	0.163*** (0.009)	0.069*** (0.008)
Placebo group (18-19 years)	-0.101*** (0.031)	-0.025 (0.026)	-0.117** (0.046)	-0.021** (0.008)	-0.012 (0.012)
Treated state * placebo group	0.044 (0.025)	-0.008 (0.087)	0.039 (0.060)	0.035** (0.015)	0.007 (0.013)
Observations	11,339	6,782	6,782	6,764	8,878

Note: Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Placebo group (1 if respondent is aged 18-19, 0 if respondent is aged 21-22). Data: Waves 1-5. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1

Compositional stability

One important limitation of the research design relates to the fact that I rely on repeated cross-sections rather than panel data. An additional identifying assumption in this case is that the composition of the target population does not change between pre- and post-treatment periods. Such compositional differences could confound the relationship between enfranchisement and political maturity since the estimated effect may be attributable to changes in the target population (Lee and Kang, 2006). It seems unlikely that voting age reforms in Germany prompted citizens to move to different states. More plausibly, unrelated migratory and demographic trends may have altered the target population between different survey waves. To test for problematic compositional changes in the target population, I estimate several two-way fixed effects (DID) regression models, using time-varying state-level covariates as outcomes (unemployment rate, log population, CDU vote share, turnout, and public education spending per capita). In this case, the unit of analysis is the state-year, and the dataset is a balanced panel for all years between 1990 and 2018. Failing to reject the null hypothesis of no treatment effect attributed to the voting age reform can be interpreted as evidence in favour of compositional stability (Wing et al., 2018). The results presented in Table B15 support the assumption of compositional stability, as none of the estimated treatment effects are statistically significant at conventional levels.

Table B15 Effect of enfranchisement on time-varying state-level covariates

Outcome	(1) Unemp. rate	(2) Log pop.	(3) CDU share	(4) Turnout	(5) Edu. spending
Voting at 16	0.465 (0.627)	-0.004 (0.026)	0.294 (0.749)	0.466 (0.644)	-42.428 (28.430)
Observations	400	416	464	400	352

Note: OLS models with state- and year fixed effects. Coefficients are shown with standard errors (clustered at the state-level) in parentheses. The treatment variable (Voting at 16) equals 1 if the voting reform has been implemented in a specific state and 0 otherwise. The dataset is a balanced state-year panel for years 1990-2018. The data on outcomes are from various sources (see Appendix C, Controls). All models include state fixed effects, year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1

Bootstrap

As an additional robustness check, I employ a wild cluster bootstrap procedure to obtain more accurate p-values. This is advisable given that in a setting with few clusters – 16 states in our case – the standard cluster-robust variance estimator may lead to over-rejection of the null and confidence intervals that are too narrow (Bertrand et al., 2004; Cameron et al., 2008). Intuitively, the procedure generates many bootstrap samples that mimic the distribution from which the original sample was obtained. It then computes a t-statistic for the coefficient of interest in each bootstrap sample. The refined p-value is the proportion of the bootstrap t-statistics that are more extreme than the t-statistic obtained from the original sample (Angrist and Pischke, 2009). In a setting with very few ‘treated’ clusters – 10 states in our case – the standard wild cluster bootstrap will typically under-reject the null of no treatment effect when the null is imposed (restricted), and over-reject when the null is not imposed (unrestricted) (MacKinnon and Webb, 2018; Roodman et al., 2019). To account for this problem, I also employ the ‘sub-cluster’ wild bootstrap procedure proposed by MacKinnon and Webb (2018), where the wild bootstrap data-generating process is clustered at a finer/lower level (i.e. state-year or individual level) than the covariance matrix (i.e. state level). Table B16 presents the results from the different wild bootstrap approaches. As expected, the standard wild cluster bootstrap procedure under-rejects (or over-rejects) when the null is imposed (or not imposed). Clustering the bootstrap errors at the finer/lower state-year or individual level, however, leads to more consistent p-values. Overall, the bootstrap results support the main findings, although as expected the bootstrap p-values tend to be more conservative than the p-values obtained from the standard cluster-robust variance estimator.

Table B16 Wild cluster bootstrap

Control age group	19-20 years			
	(1)	(2)	(3)	(4)
Outcome	Pol. interest	Ext. efficacy	Int. efficacy	Will. to vote
Treatment effect (β)	0.085	0.145	0.153	0.037
Cluster-robust SE	0.037	0.108	0.068	0.014
t-statistic	2.30	1.34	2.23	2.58
p-value	0.036	0.200	0.042	0.021
<i>p-value from:</i>				
Bootstr. by state, restricted	0.131	0.395	0.048	0.161
Bootstr. by state, unrestricted	0.084	0.403	0.177	0.040
Bootstr. by state-year, restricted	0.082	0.399	0.061	0.076
Bootstr. by state-year, unrestricted	0.083	0.393	0.145	0.029
Bootstr. by individual, restricted	0.067	0.285	0.102	0.056
Bootstr. by individual, unrestricted	0.068	0.285	0.097	0.058

Note: Note: OLS with state-, survey-year and age-group fixed effects, where the age-group indicator (1 if respondent 16-17 years old, 0 otherwise) is interacted with a treatment indicator (1 for respondents in states and survey years where voting at 16 has been implemented, 0 otherwise). β refers to the coefficient on the interaction between the treatment indicator and the age group indicator. Standard errors clustered at the state-level. Wild bootstraps are run with 9999 replications and 10 grid points. Restricted bootstraps impose the null that $\beta = 0$ and unrestricted bootstraps do not impose the null that $\beta = 0$.

Replication using SOEP data

As a robustness check, I replicate the main analysis using data on political interest from the German Socio-Economic Panel (SOEP). The SOEP is an annual, nationally representative longitudinal survey of private households, run by the German Institute for Economic Research. Beginning in 1984, SOEP has surveyed around 15,000 households and about 30,000 individuals every year, on topics including household composition, education, occupational biographies, employment, earnings, health, and satisfaction indicators.²⁵ For the analysis, only German respondents aged 16-17 and 19-20 and who completed the SOEP's individual- or youth questionnaire are kept. Interest in politics is measured by the question 'Generally speaking, how interested are you in politics?' with four possible answers ranging from 'not at all' (1) to 'very much' (4). The survey item is available for every year since 1985. Table B17 presents descriptive statistics.

Using SOEP data on interest in politics produces very similar results to the main analysis of the JS/AIDA data. Table B18 presents the regression output from the SOEP replication.

²⁵For data and documentation, see <http://companion.soep.de/>

Table B17 Descriptive statistics (SOEP)

	Dataset	Years	Obs.	Mean	SD	Min	Max	Var type
Interest in politics	SOEP	1985-2017	26610	2.00	0.78	1.00	4.00	Ordinal

Note: German respondents aged 16-17 and 19-20 years.

As in the main results, the negative coefficient on *Treated age group* indicates that disenfranchised underage youth are, on average, less politically interested than adult youth. As in the main results, the positive coefficient on the interaction suggests that this difference is reduced significantly when 16-17 year-olds are enfranchised. The estimated reduction in prior differences amounts to 34% in the SOEP data, which is comparable in size to the 41% reduction found in the JS/AIDA data.

Table B18 Effect of enfranchisement on interest in politics (SOEP)

Control age group	19-20 years
Outcome	Interest in politics
Dataset	(SOEP)
Treated state (voting at 16)	0.183 (0.124)
Treated group (16-17 years)	-0.114*** (0.012)
Treated state * treated group	0.039** (0.016)
State fixed effects	✓
Survey-year fixed effects	✓
State * survey-year FEs	✓
Cluster-robust SEs	✓
Observations	26,603
Reduction in difference between age groups	34%

Note: Note: OLS with state-, year-, and state-year fixed effects. Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). 19-20 year-olds are the control age group. The last row displays the percentage reduction in the difference between underage and adult youth attributable to voting at 16, using the estimated difference in control states as baseline. *** p<0.01, ** p<0.05, * p<0.1.

Including 18-year-olds

In the main analysis, 18-year-olds are excluded to make sure that the older control group does not include any “treated” individuals. This is a concern because 18-year-olds typically attend

the same class as 17-year-olds (who are in the treatment group), and the German voting age reforms were accompanied by school-based political education initiatives targeting the newly enfranchised. 18-year-olds are therefore likely to have been exposed to enfranchisement-related political education initiatives, either directly or indirectly through discussions with classmates. Table B19 presents results from fixed effects regression models predicting various measures of political maturity. The specification is the same as the one used for the main results (Table 3.2), except that 18-19 year-olds are used as the control age group rather than 19-20 year-olds.

As in the main results, there is evidence for an equalising effect of enfranchisement on political maturity differences between underage and adult youth. The negative coefficient on *Treated age group* indicates that disenfranchised 16-17 year-olds are, on average, less politically mature than 18-19 year-olds. As in the main results, the exception is attitudinal consistency, where there are no baseline differences between age groups to begin with. The positive coefficients on the interactions (except for attitudinal consistency) suggest that the baseline difference in political maturity between age groups is reduced when 16-17 year-olds are enfranchised. In this case, the reductions are statistically significant for willingness to vote and external efficacy, while for interest in politics and internal efficacy the reductions fall short of the conventional significance threshold.

Table B19 Effect of enfranchisement on political maturity (including 18-year-olds)

Control age group	18-19 years				
Outcome	(1)	(2)	(3)	(4)	(5)
Dataset	Pol. interest (JS/AIDA)	Ext. efficacy (JS)	Int. efficacy (JS)	Will.to vote (JS)	Att. consist. (JS/AIDA)
Treated state (voting at 16)	-0.633*** (0.029)	-0.306*** (0.041)	-0.179*** (0.030)	0.248*** (0.006)	0.117*** (0.006)
Treated group (16-17 years)	-0.190*** (0.020)	-0.115*** (0.034)	-0.204*** (0.029)	-0.085*** (0.010)	-0.006 (0.010)
Treated state * treated group	0.072 (0.051)	0.125* (0.071)	0.092 (0.053)	0.028** (0.011)	-0.033** (0.013)
Observations	12,162	7,271	7,271	7,298	9,636
Reduction diff. btw. groups	-	109%	-	32%	-

Note: OLS with state-, year-, and state-year fixed effects. Coefficients are shown with cluster-robust standard errors in parentheses. The independent variables are Treated state (1 if voting at 16 has been implemented, 0 otherwise) and Treated age group (1 if respondent is aged 16-17, 0 otherwise). 18-19 year-olds are the control age group. All models include state fixed effects, survey-year fixed effects, state*survey-year fixed effects and cluster-robust SEs. *** p<0.01, ** p<0.05, * p<0.1.

G Does the parallel trends assumption hold in the DID design?

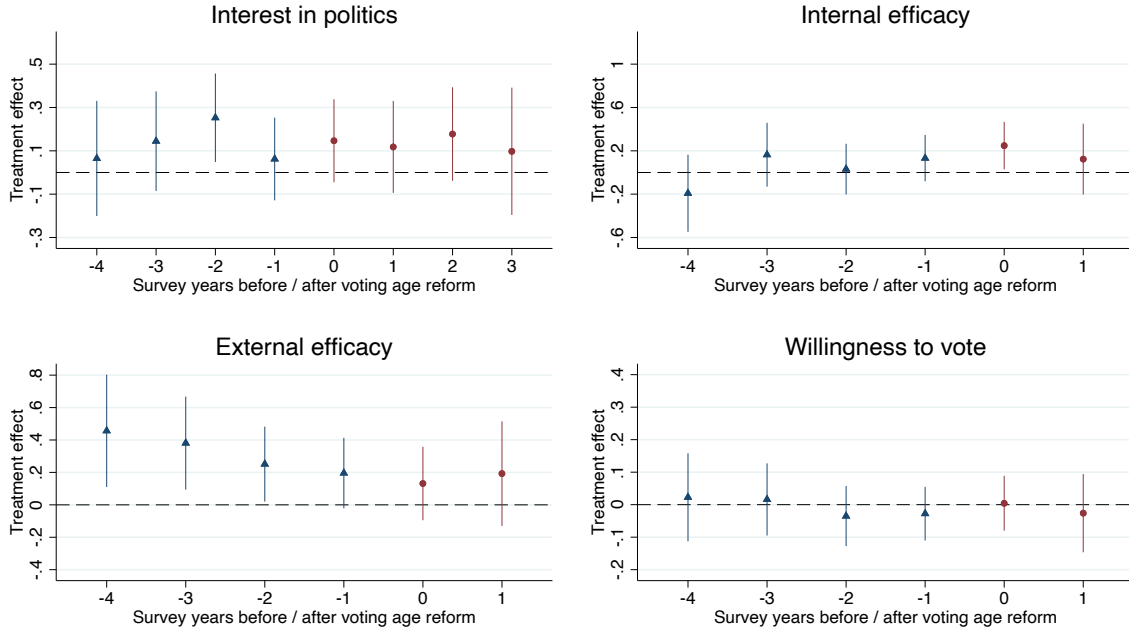
The staggered introduction of voting at 16 across German states intuitively lends itself to a generalised difference-in-differences (DID) type analysis, where state-level fixed effects account for potential bias from unobserved time-invariant state-level characteristics and year fixed-effects account for unobserved time trends shared across all states. However, in the main text, I argue that the parallel trends assumption is likely violated in the DID design, due to time-varying confounders that also vary across states (such as political mobilisation and incumbent party ideology). In this section, I provide empirical evidence to support this argument.

In a generalised DID with multiple treatment groups (states) and multiple periods (survey years) it is difficult to provide a visual inspection of parallel trends in treatment and control states prior to the voting age reform. However, we can assess the parallel trends assumption by employing an event study approach. This entails coding a binary ‘switching’ indicator equal to 1 for the first survey year after a state switched to voting at 16 (and 0 otherwise) and then including four leads and three lags of this indicator in the generalised DID model. The event study model is estimated by

$$Y_{igst} = \gamma_s + \lambda_t + D_{st}\beta + \sum_{s=1}^S D_{s,t+}\delta_s + \sum_m^M D_{s,t+}\theta_m + \epsilon_{st} \quad (3.4)$$

where Y_{igst} is a measure of individuals' political maturity, γ_s is a state-level fixed effect, λ_t is a survey-year fixed effect, D_{st} is the ‘switching’ indicator, and ϵ_{st} is the error term. The parameter β captures the immediate effect of the voting age reform on the political maturity of 16-17 year-olds, δ_s captures any ‘phase-in’ effects of the reform for S survey years prior to reform, and θ_m captures any additional effects of the reform for M survey years after the reform. The generalised DID regression model relies only on data from 16-17 year-olds, as the within-state control age group from the triple-difference type design is dropped. If the parallel trends assumption holds, the coefficients on all leads δ should be zero. Figure B7 presents estimated treatment effects on several measures of political maturity for all leads and lags (with their 95% confidence intervals). The results indicate that the parallel trends assumption is likely violated, as several of the leads (in blue) have statistically significant coefficients.

Fig. B7 Dynamic effect of enfranchisement on political maturity of underage youth



Note: Estimated treatment effect of switching to voting at 16 on outcomes of 16-17 year-olds for survey years before (blue) and after (red) the voting age reform. Point estimates are from a linear fixed effects regression with four leads and three lags of the switching indicator. Switching indicators are equal to one only in the relevant survey year. Survey year 0 refers to the first survey year after voting at 16 was implemented. Vertical lines are 95% confidence intervals. Sample: 16-17 year olds.

H Fully-saturated models

As a robustness check, I also replicate the main results using a fully saturated triple-difference model, which is estimated by

$$Y_{igst} = \gamma_s + \lambda_t + \delta_g + D_{st} + \beta(D_{st} * \delta_g) + (\gamma_s * \lambda_t) + (\gamma_s * \delta_g) + (\lambda_t * \delta_g) + \varepsilon_{igst} \quad (3.5)$$

where Y_{igst} is a measure of individuals' political maturity, γ_s is a state-level fixed effect, λ_t is a survey-year fixed effect, δ_g is an indicator equal to 1 if the respondent is aged 16-17 years and 0 if she is aged 19-20 years, D_{st} is a treatment indicator equal to 1 for respondents in states and survey years where voting at 16 has been implemented, $(\gamma_s * \lambda_t)$, $(\gamma_s * \delta_g)$ and $(\lambda_t * \delta_g)$ are pair-wise interactions between state-, survey-year and age-group fixed effects, and ε_{igst} is the error term. As in the less saturated model specification used in the main analysis (Model 3.1), the quantity of interest is β , which identifies the marginal effect of

enfranchisement on the political maturity of 16-17 year-olds relative to the slightly older control age group.

Table B20 and Table B21 below report the estimated treatment effects (β) and associated standard errors obtained from the less saturated model (Model 1) as well as the fully saturated model (Model 2, highlighted in grey) for several outcome variables. It shows that the inclusion of the two additional pair-wise interactions in Model 2 increases the standard error of β relative to Model 1 but leaves the size of the coefficient β largely unchanged. This indicates that lack of power may be the main reason that the results from the fully saturated model do not reach conventional significance levels. An exception are the results for external efficacy, where the coefficient in Model 2 is roughly half the size compared to Model 1. Finally, F-tests suggest that including the two additional pair-wise interactions does not significantly improve the explanatory power of the model regardless of the outcome variable used. The F-tests test whether the two additional pair-wise interactions in Model 2 are jointly significant. Failure to reject the null that all parameters of the two additional pair-wise interactions are equal to zero is evidence that they do not improve the explanatory power of the model. The results are as follows: Interest in politics: $F(19, 11714) = 1.32$; $\text{Prob} > F = 0.1564$. External efficacy: $F(17, 6909) = 1.56$; $\text{Prob} > F = 0.0665$. Internal efficacy: $F(17, 6909) = 1.46$; $\text{Prob} > F = 0.0976$. Willingness to vote: $F(17, 6923) = 1.15$; $\text{Prob} > F = 0.3006$.

Table B20 Effect of enfranchisement on political maturity (fully saturated model)

Control age group	19-20 years			
Outcome	Interest in politics		External efficacy	
Model	(1)	(2)	(1)	(2)
Treatment effect (β)	0.094** (0.039)	0.081 (0.070)	0.142** (0.062)	0.079 (0.099)
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Age-group indicator	✓	✓	✓	✓
State * survey-year FEs	✓	✓	✓	✓
Age group * survey-year FEs		✓		✓
Age group * state FEs		✓		✓
Observations	11,815	11,815	6,976	6,976

Note: Model 1 is an OLS model with state-, survey-year and state*year fixed effects. The age-group indicator (1 if respondent 16-17 years old, 0 otherwise) is interacted with a treatment indicator (1 for respondents in states and survey years where voting at 16 has been implemented, 0 otherwise). Model 2 (highlighted in grey) is a fully saturated triple-difference model, which is the same as Model 1 except that two additional pair-wise interactions (between age group and survey year FEs as well as age group and state FEs) are included. In Model 1, β refers to the coefficient on the interaction between the treatment indicator and the age group indicator. All models use 19-20 year-olds as the control age group. *** p<0.01, ** p<0.05, * p<0.1

Table B21 Effect of enfranchisement on political maturity (fully saturated model) – cont.

Control age group	19-20 years			
Outcome	Internal efficacy		Willingness to vote	
Model	(1)	(2)	(1)	(2)
Treatment effect (β)	0.159** (0.064)	0.167* (0.101)	0.040** (0.017)	0.031 (0.030)
State fixed effects	✓	✓	✓	✓
Survey-year fixed effects	✓	✓	✓	✓
Age-group indicator	✓	✓	✓	✓
State * survey-year FEs	✓	✓	✓	✓
Age group * survey-year FEs		✓		✓
Age group * state FEs		✓		✓
Observations	6,976	6,976	6,990	6,990

Note: Model 1 is an OLS model with state-, survey-year and state*year fixed effects. The age-group indicator (1 if respondent 16-17 years old, 0 otherwise) is interacted with a treatment indicator (1 for respondents in states and survey years where voting at 16 has been implemented, 0 otherwise). Model 2 (highlighted in grey) is a fully saturated triple-difference model, which is the same as Model 1 except that two additional pair-wise interactions (between age group and survey year FEs as well as age group and state FEs) are included. In Model 1, β refers to the coefficient on the interaction between the treatment indicator and the age group indicator. All models use 19-20 year-olds as the control age group. *** p<0.01, ** p<0.05, * p<0.1

Chapter 4

Is compulsory voting habit-forming? Regression discontinuity evidence from Brazil

Abstract: Voting in one election increases one's propensity to vote in the future. It remains unclear, however, whether this pattern holds when voting is compulsory – as is the case in a quarter of all democracies. Is compulsory voting habit-forming? I address this question using a regression discontinuity design and administrative turnout data from Brazil, where voting is voluntary at age 16 and compulsory at age 18. I find no evidence that compulsory voting instils voting habits. Instead, the evidence points to a first-time compulsory voting boost, which gradually dissipates as voters grow older. I show that targeted mobilisation of first-time compulsory voters is a plausible mechanism behind the turnout boost. Alternative explanations find less support in the data. The results clarify the scope conditions of prior research on voting habits, and have important implications for the debate over the second-order effects of compulsory voting.

Keywords: Compulsory voting, voting habits, first-time voting boost, regression discontinuity, Brazil

Note: This chapter has been published as a peer-reviewed journal article in *Electoral Studies* ([Dunaiski, 2021](#)).

4.1 Introduction

One of the most robust empirical findings in political science is that interpersonal differences in voter turnout persist over time (Campbell et al., 1960; Miller and Shanks, 1996). A common explanation for this persistence is that voting and abstaining in elections is habit-forming. Several experimental and quasi-experimental studies from the US show that voting in one election increases one's propensity to vote in the future, which is interpreted as evidence for habit formation¹ (Coppock and Green, 2016; Dinas, 2012; Fujiwara et al., 2016; Gerber et al., 2003; Meredith, 2009). However, given that these studies focus on voluntary voting, it remains unclear whether this pattern holds when voting is compulsory. This is remarkable given that a quarter of all democracies have compulsory voting systems (Birch, 2009) and compulsory voting is frequently put forward as an effective mechanism to counteract low and unequal turnout in Western democracies (Bechtel et al., 2016; Franklin et al., 2004; Lijphart, 1997). A priori, it is not obvious what to expect when voting is compulsory. Evidence from the US shows that a positive shock to the cost of voting induced by rainfall on Election Day reduces turnout contemporaneously as well as in subsequent elections, which suggests habit formation (Fujiwara et al., 2016). But can an exogenous increase in the cost of abstention induced by compulsory voting have similar knock-on effects? Or does the coercive element of compulsory voting undermine habit formation?

This paper provides first causal evidence on the long-term, individual-level turnout consequences of compulsory voting. I study a quasi-experiment in Brazil, where voting is voluntary for individuals aged 16-17 and compulsory for individuals who turn 18 before or on Election Day.² To isolate the causal effect of compulsory voting on citizens' downstream turnout,³ I employ a regression discontinuity (RD) design, which compares turnout in one election between individuals who were just old enough or just too young to be eligible for compulsory voting in the previous election. In line with prior studies on voting habits (Coppock and Green, 2016; Fujiwara et al., 2016; Meredith, 2009), I interpret positive

¹I use the term "voting habit formation" to refer to the independent and positive causal effect of having voted in one election on voting again in the future, in line with previous (quasi-) experimental studies (Coppock and Green, 2016; de Kadt, 2017; Dinas, 2012; Fujiwara et al., 2016; Gerber et al., 2003). Habit formation defined in this way does not correspond to what psychologists would refer to as habits, which are "learned sequences of acts that have become automatic responses to specific cues, and are functional in obtaining certain goals or end-states" (Verplanken and Aarts, 1999, p.104). Voting cannot be performed in a subconscious or automatic manner, so it is difficult to reconcile voting behaviour with psychological theories of habituation (Blais and Daoust, 2020; de Kadt, 2017).

²At age 70 voting becomes voluntary again.

³I borrow terminology from (Green and Gerber, 2002) to distinguish between the effect of the being compelled to vote in one election on turnout in that same (contemporaneous) election and turnout in subsequent (downstream) elections.

downstream turnout effects as evidence for habit formation.⁴ I use administrative data from the complete Brazilian voter files for all elections between 2008 and 2016. Each voter file records validated, individual-level turnout of around 150 million registered voters as well as unique identifiers that allow me to track individuals across elections.

I find no evidence that compulsory voting is habit-forming, even after several compulsory elections. Instead, the results show that the introduction of compulsory voting at age 18 is associated with a precisely estimated decrease in turnout in subsequent compulsory elections of around 1 percentage point. This negative downstream effect is observed across all municipal and general elections in the sample and withstands several robustness checks. I interpret the negative downstream effect as a first-time compulsory voting boost amongst individuals who are compelled to vote for the first time as opposed to the second time.⁵ Further analysis shows that the first-time compulsory voting boost gradually disappears as voters experience subsequent compulsory elections. Evidence on potential mechanisms suggests that targeted mobilisation of first-time compulsory voters is a plausible explanation behind the turnout boost. Alternative explanations that focus on voters' psychological response to compulsory voting find less support in the data.

The paper makes three distinct contributions. First, it makes an important empirical contribution to the voting habits literature, which has so far focused on countries with voluntary voting systems. Whilst this literature has found that voting in one election increases one's propensity to vote in the future (Coppock and Green, 2016; de Kadt, 2017; Dinas, 2012; Fujiwara et al., 2016; Meredith, 2009), I provide first causal evidence of an opposite association under compulsory voting. The paper therefore clarifies the scope conditions of prior research on voting habits and shows that previous findings do not necessarily generalise to contexts where voting is compulsory. To my knowledge, only two studies have so far examined the relationship between compulsory voting and voting habits, and found no evidence for a causal effect (Bechtel et al., 2018; Gaebler et al., 2017). However, both studies rely on aggregate-level turnout data, which raises ecological inference concerns. In particular, the null results at the aggregate level may hide significant heterogeneity in citizens' response to compulsory voting. This is important because research on political socialisation suggests that young citizens, who have not yet become habitual voters or abstainers, are likely to be more responsive to interventions such as compulsory voting compared to older citizens (Franklin and Hobolt, 2011; Franklin et al., 2004; Plutzer, 2002). To draw firm conclusions about the behavioural consequences of compulsory voting, we therefore need to analyse

⁴In this case, habit formation can be attributed to the introduction of compulsory voting at age 18.

⁵This interpretation is in line with recent quasi-experimental research from Denmark and Finland (Bhatti et al., 2016), which I discuss in more detail in the next section.

individual-level turnout data. I address this gap in the literature by providing first micro-level evidence on the relationship between compulsory voting and citizens' long-term turnout behaviour.

Second, the paper speaks to the ongoing debate over the second-order effects of compulsory voting.⁶ While some studies have found that compulsory voting can foster citizens' pro-civic orientations by increasing their political knowledge, interest, and sense of civic duty to vote (Córdova and Rangel, 2017; Feitosa et al., 2019; Sheppard, 2015; Shineman, 2012), others provide null results or evidence that compulsory voting can foster anti-system sentiments (De Leon and Rizzi, 2014; Henn and Oldfield, 2016; Holbein et al., 2020; Loewen et al., 2008; Selb and Lachat, 2009; Singh, 2019; Singh and Roy, 2018). In line with the second set of studies, the results presented here suggest that the transformative potential of compulsory voting is limited. In particular, the results cast doubts on recent proposals to make voting compulsory for first-time voters only in order to boost aggregate turnout in the long run (Birch and Lodge, 2015; Lodge and Birch, 2012) as there is no evidence that compulsory voting can instil voting habits in young people.⁷

Finally, the paper makes a theoretical contribution to the small, but rapidly growing literature on the first-time voting boost, which has found that (voluntary) turnout amongst first-time voters in several European countries is often significantly higher than turnout amongst comparable second-time voters (Bhatti and Hansen, 2012; Bhatti et al., 2016; Konzelmann et al., 2012; Zeglovits and Aichholzer, 2014). Specifically, I propose (and test) several mechanisms that can explain a first-time voting boost under compulsory voting rules. Empirically, I provide first evidence of the first-time voting boost outside of Europe and in the context of compulsory voting.

4.2 Theory and hypotheses

The voting habits theory posits that the act of voting is self-reinforcing (Aldrich et al., 2011) and that voting habits are acquired during one's first few electoral experiences (Franklin et al., 2004; Plutzer, 2002). It remains unclear, however, what the precise causal mechanisms

⁶Second-order effects refer to those effects beyond the immediate impact of compulsory voting on turnout (Fowler, 2013; Hirczy, 1994; Jaitman, 2013).

⁷No country in the world has compulsory voting for first-time voters, so it is not possible to directly test the validity of the claim that first-time compulsory voting can instil voting habits. However, I show that the introduction of compulsory voting at age 18 has no positive downstream turnout effects, despite having a strong positive impact on contemporaneous turnout. This indicates that compulsory voting has very limited or no influence on the formation of voting habits.

are through which voting in one election increases one's propensity to vote in the future. In theory, past turnout could affect any of the terms in the Downsian cost-benefit model (Fujiwara et al., 2016). Furthermore, it could influence the strategic behaviour of political parties and interest groups, for example, if active voters are more easily mobilised than inactive voters (Gerber et al., 2003).

If compulsory voting boosts turnout contemporaneously,⁸ then the voting habits theory provides several plausible mechanisms through which compulsory voting can be expected to have a positive effect on downstream turnout.⁹ First, voting in one election may lower the perceived cost of voting in subsequent elections. In their first election, voters must incur informational fixed costs such as learning how to get to the polling station, and if voters are risk averse, they will become more likely to vote once they learn about the true opportunity cost of voting (Fujiwara et al., 2016). Second, voting in one election may lead citizens to positively update their taste for voting, thereby increasing the intrinsic rewards of voting in the future. A plausible micro-foundation for this mechanism is cognitive dissonance, whereby voters adjust their taste for voting in order to create consonance with their past behaviour (Mullainathan and Washington, 2006). Third, voting in one election may increase citizens' extrinsic motivation to vote in the future, for example through social norms that reward active voters and punish abstainers (Gerber et al., 2008). The voting habits theory therefore predicts that compulsory voting has a positive effect on downstream turnout, regardless of the specific mechanism underlying the relationship.

H1: *Downstream turnout is higher (or equal) amongst citizens who were compelled to vote in a previous election compared to citizens who could vote voluntarily in a previous election, all else being equal.*

In contrast, the first-time voting boost theory predicts that first-time voters turn out at a higher rate than comparable second-time voters due to the psychological rewards of being able to vote for the first time (Bhatti et al., 2016; Zeglovits and Aichholzer, 2014). The theory draws on recent quasi-experimental evidence from Denmark and Finland, which shows that voluntary turnout amongst first-time voters can be up to 13 percentage points higher than turnout amongst comparable second-time voters (Bhatti et al., 2016). Observational studies from Germany and Austria also find evidence of a first-time voting boost (Konzelmann et al., 2012; Zeglovits and Aichholzer, 2014). Whilst there is robust empirical support for the idea

⁸I show this empirically in Appendix B.

⁹If turnout behaviour is solely determined by voting habits (and cost-benefit calculations play no role), the voting habits theory predicts zero downstream turnout effects. This is because there would be no difference in contemporaneous turnout between voluntary and compulsory voters. However, in Appendix B, I show that compulsory voting has a significant positive effect on contemporaneous turnout, which indicates that this scenario has little empirical support.

of a first-time voluntary voting boost, the causal mechanisms behind the phenomenon remain largely unexplored. Furthermore, it is unclear which mechanism, if any, could account for a first-time voting boost under compulsory voting, given that prior research has focused on voluntary voting systems.

In theory, there are at least three plausible explanations for why first-time compulsory voters can be expected to turn out at a higher rate than comparable second-time compulsory voters. First, being compelled to vote in one election may lead citizens to negatively update their taste for voting, thereby decreasing the intrinsic rewards of voting in the future. This scenario is consistent with evidence showing that compulsory voting can foster anti-system sentiments (Henn and Oldfield, 2016; Miles and Mullinix, 2019; Singh and Roy, 2018) and that extrinsic incentives to bring about a specific behaviour may decrease individuals' intrinsic motivation to engage in such behaviour (Deci et al., 1999; Gneezy et al., 2011). Second, as suggested in previous studies (Bhatti and Hansen, 2012; Bhatti et al., 2016), one's first election may elicit a certain amount of excitement or hype, regardless of whether this election is voluntary or compulsory. In Brazil, the first election for many young citizens is indeed compulsory, despite the fact that voting is voluntary for 16-17 year-olds. Holbein and Rangel (2020) estimate that only 15-20% of young Brazilians who are barely eligible to vote voluntarily actually turn out, whereas an estimated 70% of those barely eligible for compulsory voting turn out to vote.¹⁰ Finally, first-time compulsory voters may be more receptive to mobilising agents such parents, political parties or the media (Bhatti and Hansen, 2012), so that mobilisation could account for higher turnout amongst this group compared to second-time compulsory voters. Regardless of the mechanism underlying the relationship, the first-time voting boost theory predicts that compulsory voting is negatively associated with downstream turnout.

H2: *Turnout is lower amongst citizens who are compelled to vote for the second time compared to citizens who are compelled to vote for the first time, all else being equal.*

4.3 Compulsory voting in Brazil

Brazil is an ideal case study to investigate the behavioural consequences of compulsory voting. First, Brazil has used compulsory voting in all elections since 1934 (Power, 2009), so the results are unlikely to be driven by novelty effects that may confound findings on more recent electoral reforms (cf. Zeglovits and Aichholzer 2014). Second, compulsory

¹⁰Holbein and Rangel (2020) rely on voter files for the 2008-2012 elections and estimate the voting age population using data from the Brazilian National Household Survey.

voting is strictly enforced in Brazil, which is not the case in neighbouring countries such as Bolivia or Paraguay (Power, 2009). Third, Brazil's progressive open data legislation means that validated turnout data are relatively easy to access. With an electorate of more than 169 million and a population of around 206 million, Brazil is also the most populous country in the world to use compulsory voting, lending considerable statistical power to the analysis.¹¹ Most importantly, however, Brazil's electoral rules establish voting age thresholds, which can be leveraged to isolate the causal effect of compulsory voting.

Brazil's constitution mandates that registration and electoral participation is compulsory for citizens who are aged 18 to 69 years on Election Day,¹² and voluntary for those who are aged 16 to 17 years, 70 years or older, illiterate, or members of indigenous tribes. Exemptions are only granted upon request in the event of illness, travel, or to government employees. Brazilians who fail to comply with compulsory voting must justify themselves before an electoral court, or they incur a fine of R\$1.05 to R\$3.51 (approx. US\$0.25 - US\$0.85) for each missed election.¹³ Those who fail to pay the fine are barred from enrolling or renewing registration in public schools and universities, taking civil service exams, bidding for public tenders, receiving public sector salaries, requesting loans from public banks, and obtaining a passport or ID card. Registered voters who fail to vote, justify their abstention, or pay the fine in three consecutive elections are removed from the voter register and need to re-apply in person to regularise their status. Cepaluni and Hidalgo (2016) argue that the small monetary fines are unlikely to deter even poor Brazilians from abstaining. However, the prohibition on access to public services and employment appears to be effective and primarily deter well-educated Brazilians from abstaining, as they are likely to seek access to public services and employment (Cepaluni and Hidalgo, 2016).

Elections always take place on the first Sunday of October, alternating every two years between municipal and general elections for federal- and state-level positions (see Appendix A for details). Elections for president, governor, and mayor of municipalities with 200,000 or more registered voters follow a dual-ballot plurality rule, where a runoff is required between the top two candidates if no candidate receives an absolute majority in the first round. Elections for federal senator and mayor of municipalities with fewer than 200,000 registered voters follow a single-ballot plurality rule.¹⁴ Federal and state deputies as well as municipal councillors are elected using a proportional representation system. In terms of

¹¹Electorate and population figures are for 2016 and were obtained from the TSE voter file and IBGE respectively (www.ibge.gov.br).

¹²Specifically, the day of the first round election in cases where there is a run-off election.

¹³Each missed election round is counted separately. If considered ineffective, a judge may increase the maximum fine of R\$3.51 by a factor of 10.

¹⁴As of 2016, 98% of all 5,568 municipalities had fewer than 200,000 registered voters.

turnout, general and municipal elections in Brazil are very similar, with average turnout rates of around 80 percent.¹⁵

4.4 Methods and data

The empirical strategy leverages the fact that Brazil's electoral rules make voting compulsory for individuals who turn 18 before or on Election Day, whereas individuals who are slightly younger (aged 16-17) can still vote voluntarily. The causal effect of compulsory voting on downstream turnout is isolated by comparing turnout in one election (e.g. the 2016 municipal election) between individuals who are the same in all respects, except that some were just old enough or just too young to be eligible for compulsory voting in the previous election (in this case the 2014 general election).¹⁶ Given that elections always take place on the first Sunday of October every two years, there is a nearly perfect overlap between individuals who were just too young to be eligible for compulsory voting in one election and those who are eligible for compulsory voting for the first time in the next election two years later (see Appendix A for details).

In the main analysis, the treatment effect of compulsory voting eligibility on downstream turnout is estimated using a sharp RD design under the assumption that the potential outcomes of voters are continuous at the eligibility threshold (Hahn et al., 2001).¹⁷ This assumption is *a priori* plausible given that individuals cannot precisely manipulate their date of birth.¹⁸ In Appendix C, I present several falsification tests (density, covariate balance, and placebo cut-off tests) that lend additional support to the identification strategy. Point estimates are obtained by fitting local linear regressions separately on both sides of the threshold. Optimal bandwidths around the threshold are chosen to minimise the mean squared error (MSE) of the local point estimator (Cattaneo, Idrobo and Titiunik, 2018a). A triangular kernel gives

¹⁵Aggregate turnout data for previous elections are available from the TSE data repository.

¹⁶In general elections, all citizens are equally exposed to a runoff election. However, this is not the case in municipal elections due to the 200,000-population threshold rule. Consequently, individuals who were eligible to vote in a run-off election in 2008 and 2012 are excluded from the analysis of downstream effects on turnout in 2010 and 2014 respectively.

¹⁷Cattaneo, Idrobo and Titiunik (2018b) suggest that if the number of mass points in a discrete running variable (in this case birthdate) is reasonably large, it is acceptable to employ a continuity-based RD approach.

¹⁸A potential threat to causal inference are other treatments that come into effect at the age threshold (e.g. eligibility for driver's licence at age 18). However, given that elections in Brazil take place on a Sunday, these additional treatments are unlikely to have immediate effects (Cepaluni and Hidalgo, 2016). A related concern is that treatment and control groups belong to different school-year cohorts. However, this is unlikely to be a problem given that Brazil's school year starts in February and ends in December, with children typically enrolling in primary school if they turn six during their first school year.

more weight to observations closer to the threshold and robust bias-corrected confidence intervals are constructed using the approach described in (Calonico et al., 2014).

Given that compliance with compulsory voting is imperfect in Brazil, the sharp RD estimates are likely to be lower-bound estimates.¹⁹ Furthermore, the likely causal mechanisms by which compulsory voting eligibility in one election might affect downstream turnout run through its effect on contemporaneous turnout (see “Theory and hypotheses”). I therefore also employ a fuzzy RD design, which isolates the complier average causal effect (CACE) of compulsory voting on young citizens’ downstream turnout behaviour (Cattaneo, Idrobo and Titiunik, 2018b). In the fuzzy RD design, the CACE is obtained by calculating the ratio between the estimated effect of compulsory voting eligibility (treatment assignment) on downstream turnout (outcome) and the estimated effect of compulsory voting eligibility on contemporaneous turnout (treatment take-up). As in the sharp RD analysis, fuzzy RD estimates are obtained by fitting local linear regressions separately on both sides of the eligibility threshold, using triangular kernels and MSE-optimal bandwidths.²⁰ Valid causal identification requires that both potential treatment take-up and potential outcomes are continuous at the age threshold.²¹ The results from the density and covariate balance tests suggest that this assumption is plausible (Appendix C). Furthermore, valid causal identification in the fuzzy RD design requires (1) that the exclusion restriction holds, (2) that the first-stage effect is statistically significant (relevance assumption), and (3) that there are no defiers (monotonicity assumption) (Dong, 2018). Assumption (1) implies that compulsory voting eligibility in one election only affects turnout in the subsequent election through its contemporaneous turnout effect. Whilst this assumption is not directly verifiable from the data, it seems improbable that compulsory voting eligibility in one election would have a direct effect on turnout behaviour in a subsequent election. Furthermore, most of the plausible causal mechanisms by which compulsory voting eligibility in one election could affect downstream turnout run through its effect on contemporaneous turnout (see “Theory and hypotheses”). Assumption (2) in turn implies that compulsory voting eligibility has a significant effect on contemporaneous turnout. I show this empirically in Appendix B. Finally, assumption (3) implies that there are no defiers. Defiers are individuals who would vote if and only if they are assigned to the control group (eligible for voluntary voting), but would abstain if and only if they are

¹⁹An estimated 70% of those barely eligible for compulsory voting turn out to vote (Holbein and Rangel, 2020).

²⁰In the fuzzy RD design, bandwidths are MSE-optimal only for the numerator (Calonico et al., 2015).

²¹This is also referred to as the local smoothness assumption (Dong, 2018). Note that the local smoothness assumption is weaker than the local independence assumption proposed by Hahn et al. (2001).

assigned to the treatment group (eligible for compulsory voting).²² Again, we cannot directly verify this assumption, however, it seems unlikely that there are many voters in this category.

I use administrative data from the complete Brazilian voter files for all elections between 2008 and 2016. Each voter file records validated, individual-level turnout and exact date of birth of over 150 million registered voters. In addition, the voter files record individuals' gender, place of registration, biometric registration status, marital status, and education status at the time of registration. The outcome variable used in the main (sharp) RD analysis is continuous and records the average turnout rate in the first round of an election for all registered voters who share the same date of birth.²³ The running variable is individuals' date of birth, centred on those who turned 18 on the day of the first round of the election preceding the one being analysed.²⁴ Illiterates are excluded as they are not affected by compulsory voting rules. In the fuzzy RD design, the data are not collapsed to the birthdate-level and the models are instead fitted on the raw data, with standard errors clustered at the birthdate-level.²⁵ The data are not collapsed in order to enable the matching of individuals across different voter files using unique identifiers. Furthermore, the fuzzy RD analysis only includes elections since 2012, given that unique identifiers are not available for earlier elections.

4.5 Results

Contrary to the predictions of the voting habits theory, I find no evidence that the introduction of compulsory voting at age 18 has a non-negative effect on downstream turnout (Hypothesis 1). This indicates that compulsory voting fails to be habit-forming. Instead, the results show

²²In other words, the monotonicity assumption stipulates that compulsory voting eligibility either has no contemporaneous turnout effect or causes an individual to vote contemporaneously who would have otherwise abstained, but that it does not cause an individual to abstain contemporaneously who would have otherwise voted.

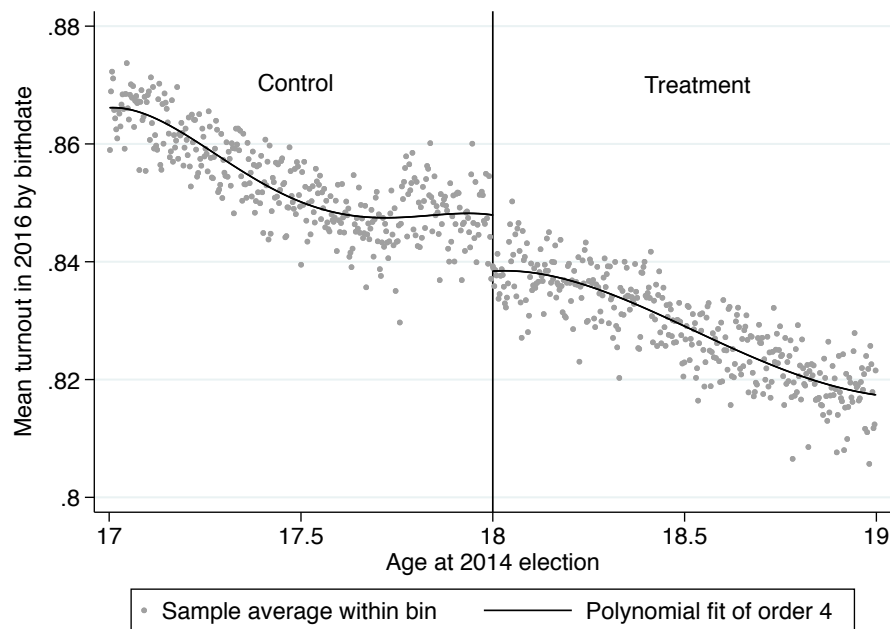
²³Following the advice of (Cattaneo, Idrobo and Titiunik, 2018b), the data are collapsed to the level of birthdate (the discrete running variable) for the sharp RD analysis. In practice, fitting a local polynomial to the collapsed data is roughly equivalent to fitting a local polynomial to the raw data with mass points (Cattaneo, Idrobo and Titiunik, 2018b), which is the approach I take in the fuzzy RD analysis. In Appendix C, I show that the main results also hold when fitting sharp RD models on the raw data and clustering the standard errors by birthdate.

²⁴All units receive a score (or running variable, forcing variable, index), where a treatment is assigned to units who score above a known threshold and not assigned to units whose score below (Cattaneo, Idrobo and Titiunik, 2018a).

²⁵(Cattaneo, Idrobo and Titiunik, 2018b) show that when the running variable is discrete and the number of mass points is relatively large (as is the case here), this approach is roughly equivalent to fitting an RD model on the collapsed data.

that the introduction of compulsory voting at age 18 has a small, but precisely estimated negative effect on turnout in subsequent compulsory elections, in line with the predictions of the first-time voting boost theory (Hypothesis 2). Figure 4.1 summarises the results graphically using data from the 2016 municipal election. It shows that individuals who were just old enough to be compelled to vote in 2014 (on the right of the threshold) have a lower propensity to vote in 2016 compared to individuals who were just too young to be eligible for compulsory voting in 2014 (on the left of the threshold), even though both groups are compelled to vote in 2016.

Fig. 4.1 Effect of compulsory voting on downstream turnout



Data: 2016 voter file. *Note:* This graph shows the effect of 2014 eligibility for compulsory voting on 2016 turnout. Curved solid lines represent 4th-order polynomial regressions of turnout in 2016 on age in 2014 fitted separately above and below the cut-off. Grey dots represent local turnout averages within evenly-spaced bins chosen to mimic the variability in the underlying data.

Table 4.1 below provides estimates of the discontinuity displayed in Figure 4.1. Columns 2 and 3 show, respectively, sharp RD estimates from a local linear regression (Model 1) and a local constant regression (Model 2). The last column (Model 3) replicates Model 1 using a different bandwidth selection procedure. The RD estimates suggest that compulsory voting eligibility in 2014 decreases turnout in 2016 by around 1 percentage point. This pattern is found in all general and municipal elections for which complete voter files are available. Figure 4.2 shows that the estimated effect of compulsory voting on downstream turnout

is consistently negative for all elections between 2008 and 2016. The difference in point estimates between 2016 and all other elections is not statistically significant, except for the difference between 2016 and 2010.²⁶ The results tables corresponding to Figure 4.2 are found in Appendix B.

Table 4.1 Effect of compulsory voting on downstream turnout

	(1)	(2)	(3)
Treatment effect	-0.008	-0.009	-0.008
Robust 95% CI	[-0.012, -0.006]	[-0.012, -0.006]	[-0.012, -0.005]
Robust p-value	0.000	0.000	0.000
Effective nr. obs. (left right)	118 119	44 45	79 80
Bandwidth	118.6	44.97	79.57
Bias bandwidth	203.4	134.9	203.4
Bandwidth selection method	MSE	MSE	CER

Note: Model 1 is a local linear MSE-optimal model. Model 2 is a local constant MSE-optimal model. Model 3 is a local linear CER-optimal model. Sharp RD estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the MSE-optimal bandwidth minimises the mean squared error of the local polynomial point estimator; the CER-optimal bandwidth minimises the coverage error rate of the robust bias-corrected confidence interval. The bias bandwidth is used to calculate the bias correction estimate. Data: 2016 voter file.

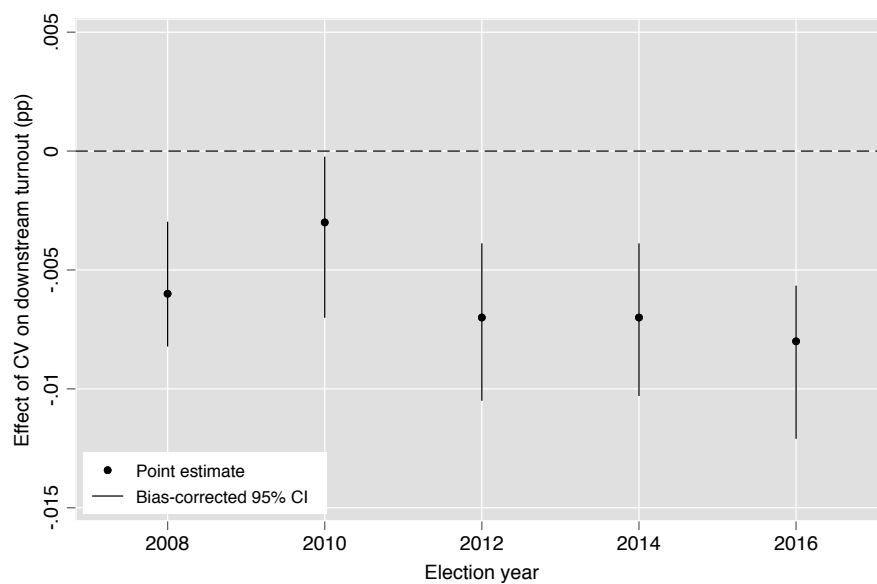
Next, Table 4.2 presents results from the fuzzy RD design, which isolates the effect of CV on downstream turnout amongst those who complied with CV when they first became eligible. The estimated CACE is consistently negative across elections and statistically significant in two out of three cases. The results indicate that the introduction of compulsory voting at age 18 decreases downstream turnout amongst compliers by 11-13 percentage points, which is around ten times the size of the average downstream turnout effect.

Several robustness checks support the main findings. First, I show that the main results are not sensitive to specific bandwidth choices (Appendix C). Second, I perform the standard pattern of RD falsification tests (density, covariate balance, and placebo cut-off tests). While the density and placebo cut-off tests are supportive of the identification strategy, the covariate balance tests provide more mixed results (Appendix C). In particular, voters' educational status appears to be unbalanced at the compulsory voting threshold in 2014. To address this concern, I show that the main results hold when controlling for educational status and when using a much smaller bandwidth of only 24 days – where educational status is balanced – together with a local randomisation RD approach (Appendix C). Third, I test whether

²⁶To calculate the standard error and 95% confidence interval around the difference between two point estimates, the sum of variances of the two point estimates is used as an estimate of the variance of the difference (see Appendix B).

the results hold unconditional on registration. Given that the voter files do not include non-registered individuals, the results may suffer from post-treatment bias if individuals' decision to register is affected by their eligibility for compulsory voting. In Appendix D, I show that the main results hold unconditional on registration by using the estimated daily turnout-to-population rate instead of the turnout-to-registration rate as the outcome variable. Finally, I confirm that the main results are not confounded by a second treatment – the introduction of voluntary voting at age 16. This is a possibility given that elections always take place on the first Sunday of October every two years and voting becomes compulsory exactly two years after 16-year-olds gain the right to vote voluntarily. To address this concern, I leverage the fact that some voters are eligible for voluntary voting twice before becoming eligible for compulsory voting. In Appendix E, I show that voluntary voting at age 16 has no discernible effect on citizens' downstream turnout behaviour, which suggests that the main results are not confounded by this potential second treatment.²⁷

Fig. 4.2 Effect of compulsory voting on downstream turnout (all elections)



Data: 2008-2016 voter files. *Note:* Sharp RD estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Vertical lines represent robust bias-corrected 95% confidence intervals, which are constructed around the bias-corrected point estimate, so they are not necessarily symmetric around the conventional point estimate. Estimates for general elections 2010 and 2014 exclude individuals who were eligible to vote in a run-off municipal election in 2008 and 2012 respectively. The corresponding results tables are found in Appendix B.

²⁷This is akin to showing that voluntary voting does not instil voting habits and that there is no first-time voluntary voting boost in Brazil.

Table 4.2 Fuzzy RD estimates of downstream turnout effect – 2012-2016

	(1) 2016 - Municipal	(2) 2014 - General	(3) 2012 -Municipal
First-stage estimate	0.0625	0.0504	0.0416
Robust standard error	0.00291	0.00199	0.00218
Treatment effect (CACE)	-0.116	-0.035	-0.133
Robust p-value	(0.028)	(0.281)	(0.038)
Observations	11,640,762	10,148,627	14,227,078
Bias bandwidth	106.4	188.2	191.4
Bandwidth	42.60	120.1	62.51
Eff. nr. obs. R	216397	579580	405413
Eff. nr. obs. L	147546	449321	301878
Robust 95% CI R	-0.0125	0.0454	-0.00752
Robust 95% CI L	-0.222	-0.156	-0.271

Note: The binary dependent variable is individual-level turnout. The CACE represents the estimated effect of introducing compulsory voting in the previous election on turnout in the current election for individuals who complied with compulsory voting in the previous election. Fuzzy RD estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Robust 95% confidence intervals are bias-corrected. Standard errors are clustered at the birthdate-level. The estimate for the 2014 general election excludes individuals who were eligible to vote in a run-off municipal election in 2012.

4.5.1 Does the first-time boost disappear over time?

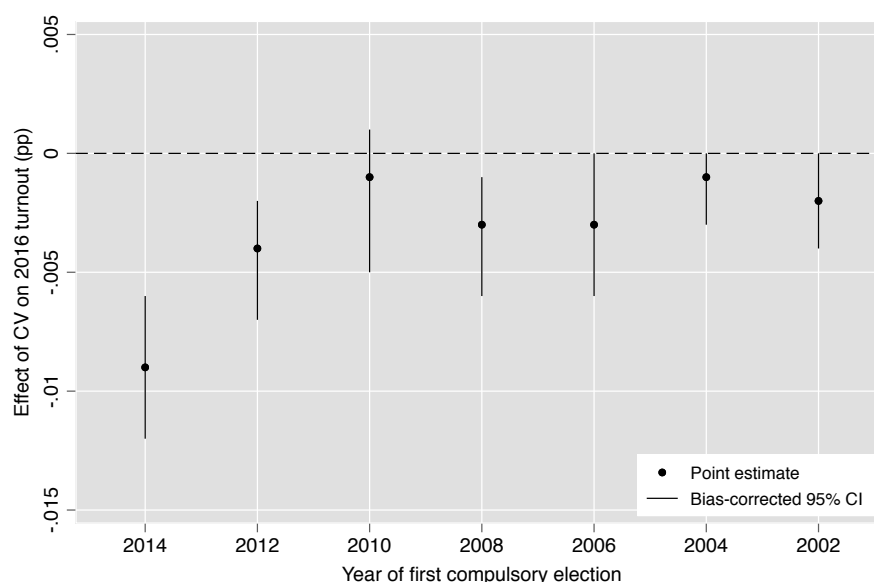
In theory, turnout rates between first-time and second-time eligibles for compulsory voting should become indistinguishable over time, as the two groups grow older and the novelty effect of being compelled to vote gradually disappears. To assess whether the first-time compulsory voting boost disappears over time, I examine how the downstream turnout effect of compulsory voting develops over several elections. Figure 4.3 displays the effect of compulsory voting on downstream turnout in 2016 for cohorts who were barely (in-)eligible for compulsory voting in elections going back until 2002.²⁸ The results indicate that the first-time compulsory voting boost does indeed disappear after around two elections, given that the estimates become indistinguishable from zero thereafter.²⁹ Figure 4.3 also provides further evidence that compulsory voting does not appear to be habit-forming. This is because

²⁸A necessary assumption is that there are no significant differences between cohorts who became eligible for compulsory voting between 2002 and 2016.

²⁹An exception is the estimate for 2008.

the estimates remain close to zero and never cross over into the positive, as one would expect if compulsory voting were habit-forming.³⁰

Fig. 4.3 Effect of compulsory voting by year of first compulsory election



Data: 2016 voter file. *Note:* This graph shows the estimated effect of compulsory voting on 2016 turnout for cohorts who were barely (in-)eligible for compulsory voting in elections going back until 2002. Exact dates for all elections since 2002 are found in Appendix A. Sharp RD estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Vertical lines represent robust bias-corrected 95% confidence intervals, which are constructed around the bias-corrected point estimate, so they are not necessarily symmetric around the conventional point estimate.

4.5.2 Why is there a first-time compulsory voting boost?

The first-time voting boost theory provides several plausible explanations for the negative association between compulsory voting and downstream turnout. These can be further explored using the available data. Mobilisation is one potential mechanism behind the first-time compulsory voting boost in Brazil. Previous studies from countries with voluntary voting rules have attributed the first-time voting boost to first-time voters' increased receptiveness

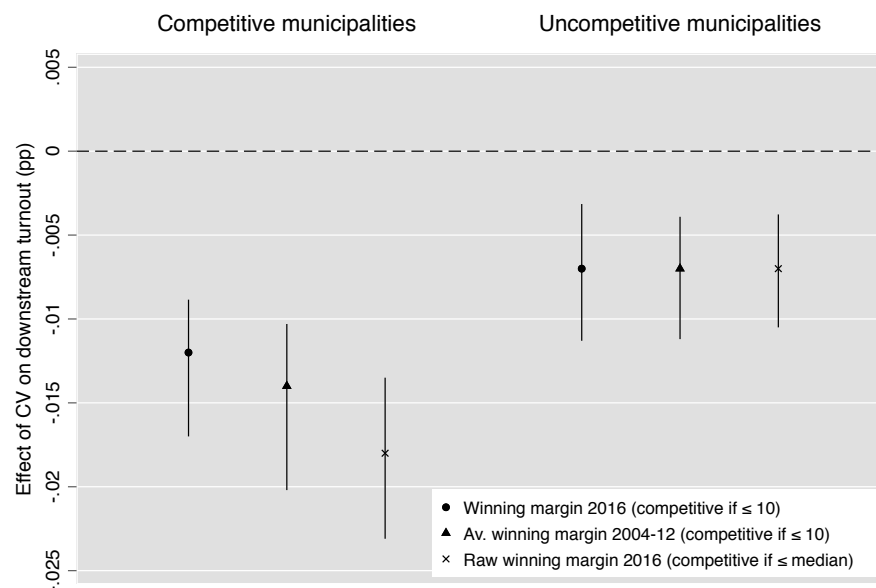
³⁰There is little reason to assume that the first-time voting boost would persist for more than two or three election cycles (Bhatti et al., 2016), whereas prior research on voting habits shows that voting in a single election can affect downstream turnout several decades later (Dinas, 2012). If compulsory voting has any positive effects on downstream turnout, the estimates should therefore eventually cross over into the positive, which they do not do.

to election-related information and mobilising agents such as the media, political parties, parents and friends (Bhatti and Hansen, 2012; Bhatti et al., 2016). This explanation stands in the tradition of the mobilisation theory of voter turnout, which argues that citizens decide to vote because their families and peers do so, or because they are mobilised by political campaigns (Gerber et al., 2008; Green et al., 2003; Rosenstone and Hansen, 1993).

Both street campaigns and media ads are important types of campaigning in Brazil, and although television and radio airtime is free during the election period, candidates spend significant resources on producing campaign ads (Avis et al., 2018). If mobilisation by political campaigns plays a role in explaining the first-time compulsory voting boost in Brazil, then we might expect the turnout boost to be more pronounced in closely contested elections, given that campaigns are likely to focus their mobilisation efforts on marginal seats (Cox and Munger, 1989). To assess whether this is the case, I disaggregate the RD analysis by competitive and uncompetitive municipalities, using three different measures of electoral competition: the percentage winning margin, the expected closeness, and the raw winning margin (see Appendix F for details). The results are summarised in Figure 4.4, which shows that the first-time compulsory voting boost is roughly twice as large in competitive municipalities compared to uncompetitive municipalities. This may indicate that targeted mobilisation plays a role in explaining the first-time compulsory voting boost in Brazil.

Psychological mechanisms may also play a role. First, one's first election may elicit a certain amount of excitement or hype, regardless of whether this election is voluntary or compulsory (Bhatti et al., 2016). Second, being compelled to vote in one election may lead citizens to negatively update their taste for voting, thereby decreasing the intrinsic rewards of voting in the future. Unfortunately, the voter file data do not allow us to explore these potential mechanisms in detail. However, in Appendix E, I show that there is no comparable first-time voting boost amongst voluntary voters, which casts some doubts on the first explanation. In relation to the second explanation, Holbein and Rangel (2020) recently showed that compulsory voting in Brazil has precisely estimated null-effects on young citizens' political interest, knowledge, social awareness, and civic engagement. If we accept these measures as suitable proxies for individuals' intrinsic motivation to vote, then the null results cast some doubts on the idea that compulsory voting leads citizens to negatively update their taste for voting. However, there is also evidence that compulsory voting can trigger anger (Miles and Mullinix, 2019) and exacerbate anti-democratic sentiments in citizens (Singh and Roy, 2018), so the idea of negative updating cannot be dismissed entirely.

Fig. 4.4 First-time boost is stronger in competitive municipalities



Data: 2016 voter file. *Note:* This graph shows the effect of 2014 eligibility for compulsory voting on 2016 turnout separately for voters registered in competitive and uncompetitive municipalities. Municipalities are coded as competitive if 1) the winning margin of the leading mayoral candidate in the 2016 municipal election is ≤ 10 percentage points; 2) the average winning margin in municipal elections 2004–2012 is ≤ 10 percentage points; 3) the raw winning margin in 2016 is \leq the median raw winning margin in 2016 (806 votes). Sharp RD estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Vertical lines represent robust bias-corrected 95% confidence intervals, which are constructed around the bias-corrected point estimate, so they are not necessarily symmetric around the conventional point estimate.

4.5.3 Who is mobilised?

Recent research suggests that voter mobilisation can happen along various dimensions simultaneously (Bechtel and Schmid, 2020; Gerber et al., 2013; Hodler et al., 2015). This raises the question of what types of voters are mobilised in the first-time compulsory voting boost in Brazil. The voter files include some individual-level covariates (educational status and gender) that allow me to explore this issue in more detail.³¹ In Appendix G, I show that the first-time compulsory voting boost is more pronounced amongst better educated, male citizens,³² which may indicate that these socio-economic groups are particularly receptive

³¹Marital status is also recorded, but only around 2.5% of all 16–24 year olds are married (as of 2016).

³²Better educated, male citizens are what Bechtel and Schmid (2020) refer to as “likely voters”. Education is a strong predictor of turnout (Lindgren et al., 2019; Wolfinger and Rosenstone, 1980). There is also evidence of a persistent gender gap in turnout, especially in second-order elections (Kostelka et al., 2019).

to mobilisation efforts in their first compulsory election.³³ The findings qualify previous research by [Cepaluni and Hidalgo \(2016\)](#) who find that CV in Brazil has a stronger mobilising effect on contemporaneous turnout amongst the better educated. My results indicate that this unequal mobilisation due to CV may only be short-lived and primarily affects first-time compulsory voters, as demobilisation in downstream elections is also more pronounced amongst the better educated.

4.6 Discussion

Several experimental and quasi-experimental studies from countries with voluntary voting rules suggest that voting is habit-forming ([Coppock and Green, 2016](#); [de Kadt, 2017](#); [Dinas, 2012](#); [Gerber et al., 2003](#); [Meredith, 2009](#)). Estimates from the US indicate that voluntary voting in one election increases turnout in downstream elections by around 5-10 percentage points, which is interpreted as evidence for habit formation ([Coppock and Green, 2016](#)). In this paper, I leverage voting age discontinuities in Brazil's electoral rules to show that this pattern does not hold when voting is compulsory. The results highlight the scope conditions of existing research on voting habits and suggest that previous findings do not generalise to contexts where voting is compulsory. This is important given that around a quarter of all democracies have compulsory voting systems ([Birch, 2009](#)) and compulsory voting is frequently proposed as a solution to low and unequal turnout in Western democracies ([Bechtel et al., 2016](#); [Franklin et al., 2004](#); [Lijphart, 1977](#)). While the findings from Brazil may not necessarily generalise to all countries with compulsory voting systems – given that enforcement and penalties vary widely ([Singh, 2011](#)) – they pertain directly to nearby countries such as Argentina and Ecuador, which use the same compulsory voting age thresholds as Brazil ([Singh, 2019](#)).³⁴

Using validated, individual-level turnout data from the Brazilian voter files, I find no evidence that compulsory voting is habit-forming, even after several compulsory elections. This corroborates previous aggregate-level findings from Switzerland and Austria, where ecological inference concerns have prevented firm conclusions ([Bechtel et al., 2018](#); [Gaebler et al., 2017](#)). In addition, however, the evidence presented in this paper points to a first-time compulsory voting boost. Individuals who are compelled to vote for the second time are less likely to turn out than comparable individuals who are compelled to vote for the first time.

³³It is conceivable that the first-time compulsory voting boost is also driven by 'reluctant' voters, who are particularly prone to cast blank or invalid ballots ([Singh, 2019](#)). Unfortunately, the Brazilian voter files do not record invalid votes, so it is not possible to test this hypothesis using the available data.

³⁴Note that in Ecuador voting is voluntary for over-65 year-olds.

This negative association between compulsory voting and downstream turnout is precisely estimated and observed across all general and municipal elections between 2008 and 2016. At around ten percentage points, the estimated first-time compulsory voting boost amongst compliers is comparable in size to the positive downstream turnout effects attributed to voting habits in the US (Coppock and Green, 2016) as well as the first-time voluntary voting boost of around 13 percentage points previously found in Denmark and Finland (Bhatti et al., 2016).³⁵ Furthermore, by focusing only on the effect of CV at the age 18 threshold, we may in fact underestimate the overall mobilising effect of CV amongst first-time voters in Brazil. Using a similar RD design, Turgeon and Blais (2019) find evidence for a significant additional turnout boost amongst voters who turn 18 just before the end of the election year, which they attribute to ignorance amongst Brazilians about when the age criterion for CV applies.³⁶ Turgeon and Blais (2019) also suggest that ignorance about the age criterion is more prevalent amongst less educated voters, which implies that the end-of-the-year turnout boost to some extent equalises the turnout differences between education groups identified at the age 18 threshold (see Appendix G).

How can the findings from Brazil be reconciled with prior research on voting habits from the US? One plausible explanation relates to differences between countries in terms of institutional barriers to voting (Melton, 2014). In the US, for example, newly enfranchised citizens face significant barriers to enrolling on the voter register (Holbein and Hillygus, 2016). Once completed, voter registration can therefore be perceived as an investment in future electoral participation, which may increase the turnout propensity of second-time eligibles relative to first-time eligibles (Bhatti et al., 2016). Whilst the first-time voting boost may also be present in the US electorate,³⁷ the existence of significant institutional barriers to voting means that previous electoral experiences (i.e. voting habits) play a much more important role in predicting turnout amongst young US citizens (Melton, 2014). In contrast, young Brazilians are compelled to register and participate in elections as soon as they turn 18, which means that the perceived cost of voting (or abstaining) should be roughly the same for first-time and second-time compulsory voting eligibles. Compulsory voting might therefore equalise some of the age differentials in youth turnout that previous studies from the US have attributed to voting habit formation (Coppock and Green, 2016; Meredith, 2009; Plutzer, 2002).

³⁵Coppock and Green (2016) estimate the CACE whilst Bhatti et al. (2016) estimate the ATE.

³⁶Many Brazilian voters appear to mistakenly believe that CV eligibility is determined by the year of birth rather than the date of birth. Turgeon and Blais (2019) estimate that nearly a third of the contemporaneous turnout effect of CV can be attributed to voter ignorance about when the age criterion for CV applies.

³⁷For example, there is evidence of a first-time voluntary voting boost in the Californian electorate (see <https://www.washingtonpost.com/news/the-fix/wp/2015/01/17/the-remarkable-california-turnout-curve/>)

An alternative explanation for the divergent findings from the US and Brazil is that the coercive element of compulsory voting leads young citizens to negatively update their taste for voting, thereby undermining habit formation in the Brazilian electorate. This explanation has intuitive appeal. However, the available evidence on the link between compulsory voting and citizens' intrinsic motivation to vote remains limited and has so far been inconclusive (cf. [Feitosa et al. 2019](#); [Holbein and Rangel 2020](#)). Further research is necessary to establish which psychological mechanism, if any, can best explain why compulsory voting is negatively associated with downstream turnout. There also remains a concern that individuals' age at their first compulsory election represents a second treatment that could explain some of the turnout differences between first-time and second-time compulsory voters in Brazil. Unfortunately, the RD design using voting age discontinuities does not allow me to address this methodological concern in a satisfactory manner, so the results should be interpreted with this limitation in mind.³⁸

Finally, the findings have important implications for the ongoing debate over the second-order effects of compulsory voting. The idea that compulsory voting can have positive externalities (beyond the well-established immediate impact on turnout) is frequently put forward by academics and policymakers arguing for the introduction of compulsory voting in the US and other Western democracies. For example, in his well-known presidential address to the American Political Science Association, Arend Lijphart argued that the introduction of compulsory voting in the US would “stimulate stronger participation and interest in other political activities” ([Lijphart 1997](#), p.10). Similarly, President Obama spoke out in favour of compulsory voting, arguing that “it would be transformative if everybody voted – that [it] would counteract money more than anything.”³⁹ These optimistic accounts, however, contrast markedly with more recent academic contributions, which suggest that compulsory voting has no effect on citizens' pro-civic orientations ([De Leon and Rizzi, 2014](#); [Holbein and Rangel, 2020](#); [Loewen et al., 2008](#)) and may even exacerbate anti-system sentiments ([Miles and Mullinix, 2019](#); [Singh and Roy, 2018](#)). While the debate is far from being settled, the results presented here give further credence to the sceptics and suggest that the transformative potential of compulsory voting may be limited. In particular, the results cast doubts on the idea that compulsory voting for first-time voters only could boost aggregate turnout in the long run ([Birch et al., 2015](#); [Lodge and Birch, 2012](#)), as there is no evidence that compulsory voting can instil voting habits in young people.

³⁸This limitation also pertains to previous studies on habit formation under voluntary voting that rely on voting age discontinuities ([Coppock and Green, 2016](#); [Dinas, 2012](#); [Meredith, 2009](#)).

³⁹CNN Politics, “Obama: Maybe it’s time for mandatory voting”, <http://edition.cnn.com/2015/03/19/politics/obama-mandatory-voting/>.

4.7 Appendix

A Election dates

Elections in Brazil always take place on the first Sunday of October, alternating every two years between municipal and general elections for federal- and state-level positions. Elections for president, governor, and mayor of municipalities with 200,000 or more registered voters follow a dual-ballot plurality rule, where a runoff is required between the top two candidates if no candidate receives an absolute majority in the first round. Runoff elections always take place on the last Sunday of October. Elections for federal senator and mayor of municipalities with fewer than 200,000 registered voters instead follow a single-ballot plurality rule. Federal and state deputies as well as municipal councillors are elected using a proportional representation system. Voter registration closes 151 days before the election. Registered voters are removed from the voter rolls if they fail to vote, justify their abstention or pay a fine in three consecutive election rounds. Elected Presidents assume office on the 1st of January following the general election.

Table C1 Election dates 2002-2018

Election type	Round	Day	Month	Year	Registration	President
General	1	06	10	2002	08/05/2002	Lula
General	2	27	10	2002		
Municipal	1	03	10	2004	05/05/2004	
Municipal	2	31	10	2004		
General	1	01	10	2006	03/05/2006	
General	2	29	10	2006		
Municipal	1	05	10	2008	07/05/2008	Rousseff
Municipal	2	26	10	2008		
General	1	03	10	2010	05/05/2010	
General	2	31	10	2010		
Municipal	1	07	10	2012	09/05/2012	
Municipal	2	28	10	2012		
General	1	05	10	2014	07/05/2014	
General	2	26	10	2014		
Municipal	1	02	10	2016	04/05/2016	
Municipal	2	30	10	2016		
General	1	07	10	2018	09/05/2018	Temer
General	2	28	10	2018		

Note: As of writing, complete voter files are unavailable for elections highlighted in grey. Temer assumed office in August 2016 after his predecessor Dilma Rousseff was impeached.

B Results tables

Table C2 Effect of compulsory voting on *contemporaneous* turnout - all elections

Election year	(1) 2016	(2) 2014	(3) 2012	(4) 2010	(5) 2008
Treatment effect	0.053	0.061	0.059	0.041	0.038
Robust SE	0.00149	0.00199	0.00188	0.00241	0.00151
Robust p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2,920	2,923	2,923	2,923	2,923
Bias bandwidth	161.9	138.1	175.2	131.5	174
Bandwidth	76.80	50.89	73.50	56.58	81.48
Eff. nr. obs. R	77	51	74	57	82
Eff. nr. obs. L	76	50	73	56	81
Robust 95% CI R	0.0555	0.0640	0.0618	0.0451	0.0398
Robust 95% CI L	0.0497	0.0562	0.0544	0.0357	0.0339
Difference v 2016		0.008	0.006	0.012	0.015
Robust SE		0.002445	0.002389	0.002701	0.002124
p-value		<0.05	<0.05	<0.05	<0.05

Note: Point estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Robust 95% confidence intervals are bias-corrected. To compute the standard error and confidence interval around the difference between the point estimates, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

Table C3 Effect of compulsory voting on *downstream* turnout - all elections

Election year	(1) 2016	(2) 2014	(3) 2012	(4) 2010	(5) 2008
Treatment effect	-0.008	-0.007	-0.007	-0.003	-0.006
Robust SE	0.00165	0.00164	0.00170	0.00173	0.00134
Robust p-value	(0.000)	(0.000)	(0.000)	(0.036)	(0.000)
Observations	2,920	2,921	2,923	2,921	2,923
Bias bandwidth	203.4	238.1	215.2	247.4	195.8
Bandwidth	118.6	140.7	134.9	158.3	124.5
Eff. nr. obs. R	119	141	135	159	125
Eff. nr. obs. L	118	140	134	158	124
Robust 95% CI R	-0.00566	-0.00388	-0.00388	-0.000236	-0.00297
Robust 95% CI L	-0.0121	-0.0103	-0.0105	-0.00701	-0.00822
Difference v 2016		0.001	0.001	0.005	0.002
Robust SE		0.002342	0.002383	0.002457	0.002116
p-value		>0.05	>0.05	<0.05	>0.05

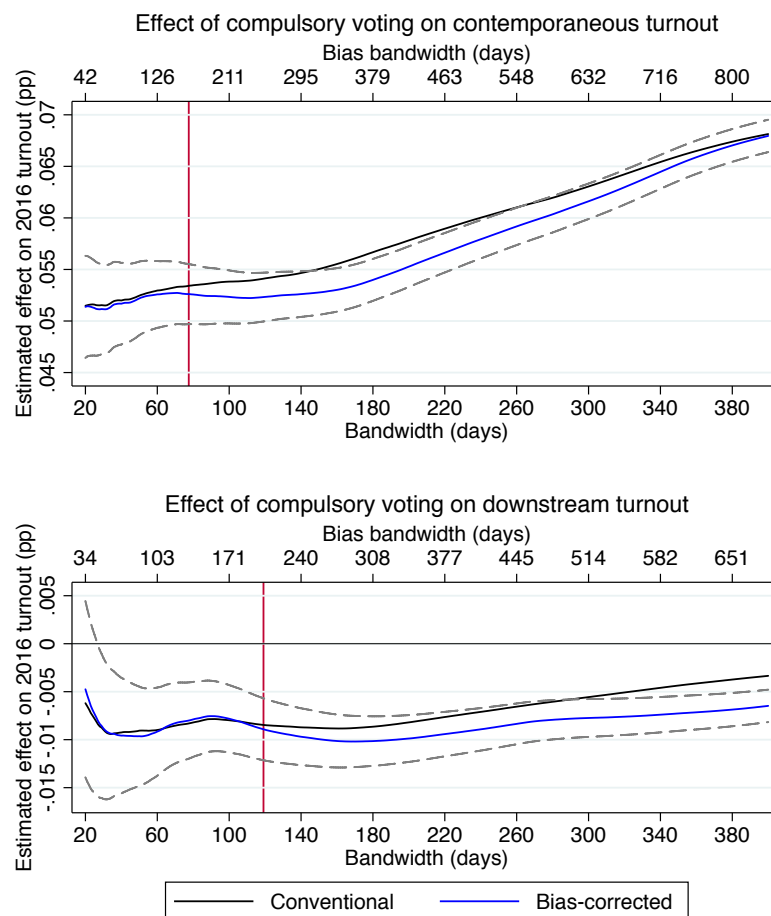
Note: Point estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Robust 95% confidence intervals are bias-corrected. Estimates of downstream effects for general elections 2010 and 2014 exclude individuals who were eligible to vote in a run-off municipal election in 2008 and 2012 respectively. To compute the standard error and confidence interval around the difference between the point estimates, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

C Robustness checks

Sensitivity to alternative bandwidths

To examine whether the main results are sensitive to alternative bandwidth choices, Figure C1 shows how RD estimates from local linear regressions on the 2016 data vary for all bandwidths between 20 and 400 days. The estimated contemporaneous effect of compulsory voting is consistently positive and statistically significant across the range of alternative bandwidths, varying between 5 and 7 percentage points. The estimated downstream effect of compulsory voting is consistently negative and only becomes indistinguishable from zero at extremely small bandwidths of less than 30 days. Bandwidth sensitivity results for all other available elections are very similar to the results from 2016 and available on request.

Fig. C1 Sensitivity to alternative bandwidths – 2016 data



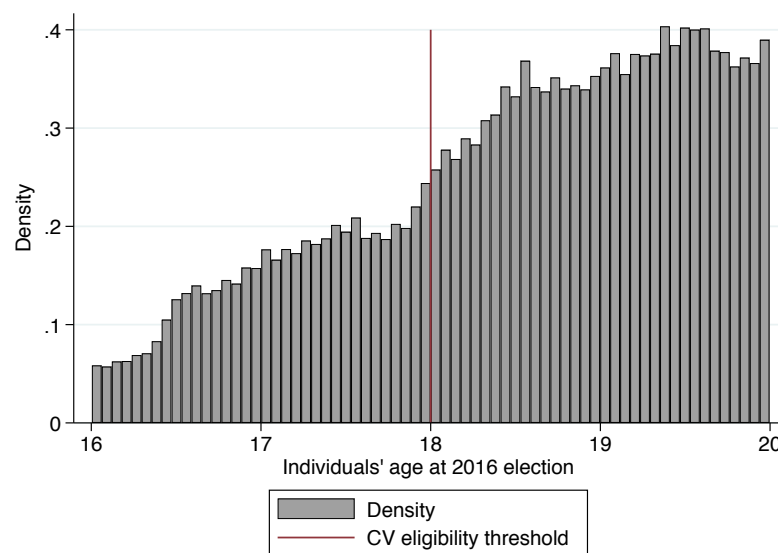
Data: 2016 voter file. *Note:* The figure displays the estimated effect of compulsory voting eligibility in 2014 on contemporaneous and downstream turnout for various bandwidths. Conventional (solid black line) and bias-corrected (solid blue line) point estimates are from local linear regressions with triangular kernel; the vertical red line marks the MSE-optimal bandwidth; dashed lines show robust bias-corrected 95% confidence intervals. Robust bias-corrected confidence intervals are constructed around the bias-corrected point estimate, so they are not necessarily symmetrical around the conventional point estimate.

Density test of the running variable

A typical falsification test in RD analysis examines whether the number of observations just above the cut-off is “surprisingly” different from the number of observations just below the cut-off (Cattaneo, Idrobo and Titiunik, 2018a). Using data from the 2016 municipal election, a visual inspection of the density of the running variable (birthdate) around the compulsory voting eligibility thresholds (at age 18 in 2016 and 2014) suggests no obvious

sorting of units to avoid or receive treatment (see Figures C2-C3 below). This makes intuitive sense, as it is improbable that individuals (or their parents) would precisely manipulate their date of birth to avoid being eligible for compulsory voting in a specific election. However, Figure C2 does suggest an overall upward trend in the number of individuals included in the voter file, which may be because older individuals are more likely to make themselves known to the state for administrative reasons (e.g. because 18-year-olds need to prove their voter file status is regular in order to enrol in university or renew their passports). To assess whether different registration rates between treatment and control groups might bias the main RD results on the contemporaneous turnout effect of CV, I replicate the analysis using the turnout-to-population rate instead of the turnout-to-registration rate as outcome variable. The results are presented in Table C9 in Appendix D and indicate that the overall upward trend in registrants (depicted in Figure C2) is unlikely to bias the main RD results.

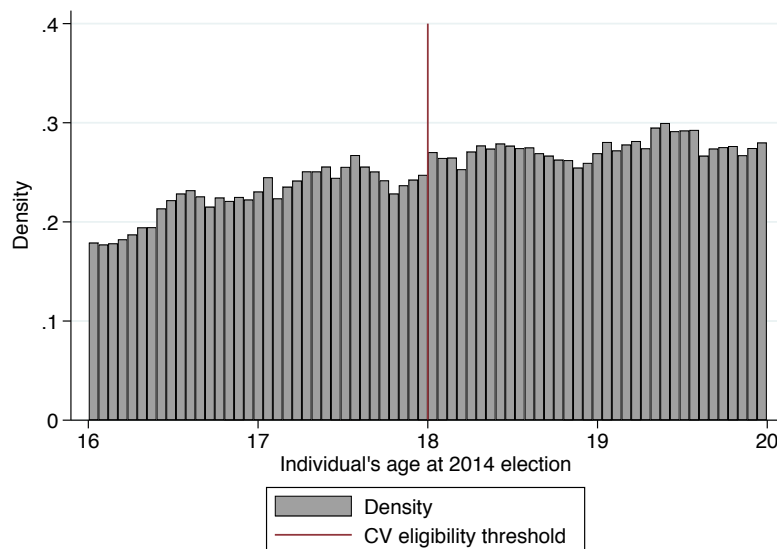
Fig. C2 Density plot at 18+ threshold in 2016 – 2016 data



Data: 2016 voter file. *Note:* The range of the running variable is restricted to 2 years around the threshold.

To formally test whether there is a disproportionate number of individuals just above the thresholds relative to those just below the thresholds (or vice versa), I employ an automated manipulation test using local polynomial density estimation (Cattaneo et al., 2017). At the 18+ threshold in 2016 (used in the RD analysis of contemporaneous turnout effects) the test fails to reject the null hypothesis of no discontinuity in the density of the running variable

Fig. C3 Density plot at 18+ threshold in 2014 – 2016 dat



Data: 2016 voter file. *Note:* The range of the running variable is restricted to 2 years around the threshold.

($p = 0.25$), which is evidence against sorting.⁴⁰ However, the same test rejects the null ($p = 0.003$) at the 18+ threshold in 2014. In other words, individuals who were just old enough to be eligible for compulsory voting in 2014 appear to be disproportionately represented in the 2016 voter file compared to those who were just too young to be eligible for compulsory voting in 2014.

One explanation for this discontinuity is that registration rates differ systematically between the two groups of young people. Even though both groups are legally required to be registered for the 2016 election (given that compulsory voting applies to both), only the slightly older group was already required to be registered for the previous election. It may be that the additional two years in between the two elections as well as penalties imposed on those who did *not* register in 2014 (despite being legally required to do so), increased the likelihood of individuals in the slightly older group to be registered in 2016. To address this concern, I employ administrative data on daily birth totals in Brazil to estimate the number of *unregistered* citizens in treatment and control groups (see Appendix D). The results suggest that the main RD estimates are not subject to differential registration bias.

Another explanation for the discontinuity at the 18+ threshold in 2014 is that systematic manipulation of the running variable score occurred. In this case, it is natural to assume that

⁴⁰Density tests for other elections included in the robustness checks are available on request.

the units closest to the threshold are most likely to be affected by such sorting (Cattaneo, Idrobo and Titiunik, 2018a). To address this concern, I run several “donut hole” RD models – where units that are very close to the threshold are excluded from the analysis. The results from the donut hole RD models (Table C4) show that the main results are robust to excluding observations in close proximity to the threshold.

Table C4 Donut hole RD estimates of downstream turnout effect – 2016 data

	(1)	(2)	(3)	(4)
Exclusion bandwidth	5 days	10 days	15 days	20 days
Treatment effect	-0.009	-0.008	-0.006	-0.006
Robust p-value	(0.000)	(0.000)	(0.028)	(0.045)
Observations	2,911	2,901	2,891	2,881
Bias bandwidth	194.8	199.8	212.2	199.2
Bandwidth	112.8	111.9	113.3	106
Eff. nr. obs. R	108	102	99	86
Eff. nr. obs. L	108	102	99	86
Robust 95% CI R	-0.00626	-0.00448	-0.000660	-0.000135
Robust 95% CI L	-0.0130	-0.0128	-0.0118	-0.0123

Note: Point estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Robust 95% confidence intervals are bias-corrected.

Covariate balance tests

Another common falsification test in RD analysis examines whether treated units (just above the threshold) and control units (just below the threshold) are similar in terms of observable, pre-determined covariates. If units do not have precise control over their score on the running variable, there should be no difference between units just above and below the threshold, except for their treatment status (Cattaneo, Idrobo and Titiunik, 2018b). Suitable covariates recorded in the voter files are individuals’ gender, biometric registration status and educational status at the time of registration.⁴¹ Using data from the 2016 municipal election, Table C5 presents sharp RD estimates of the effect of compulsory voting eligibility in 2016 and 2014 on predetermined covariates (aggregated to birthdate).⁴² The results show that the

⁴¹Marital status is also recorded in the voter files. However, I do not conduct balance tests on this variable, as only 2.5% of all 16-24 year olds in the 2016 voter file are married.

⁴²For balance tests, the CER-optimal bandwidth selection procedure is the most appropriate, given that we are primarily interested in testing the null hypothesis of no discontinuity at the cut-off (Cattaneo, Idrobo and Titiunik, 2018a).

null hypothesis of no discontinuity cannot be rejected in 4 out of 8 tests, which suggests covariate balance in those cases. Educational and biometric status, however, appear to be unbalanced at the 18+ threshold in 2014, although the difference between treatment and control groups is in both cases relatively small. Given that educational status may be related to turnout behaviour in Brazil (Cepaluni and Hidalgo, 2016), this imbalance at the threshold might bias the results.

Table C5 Sharp RD estimates for predetermined covariates – 2016 data

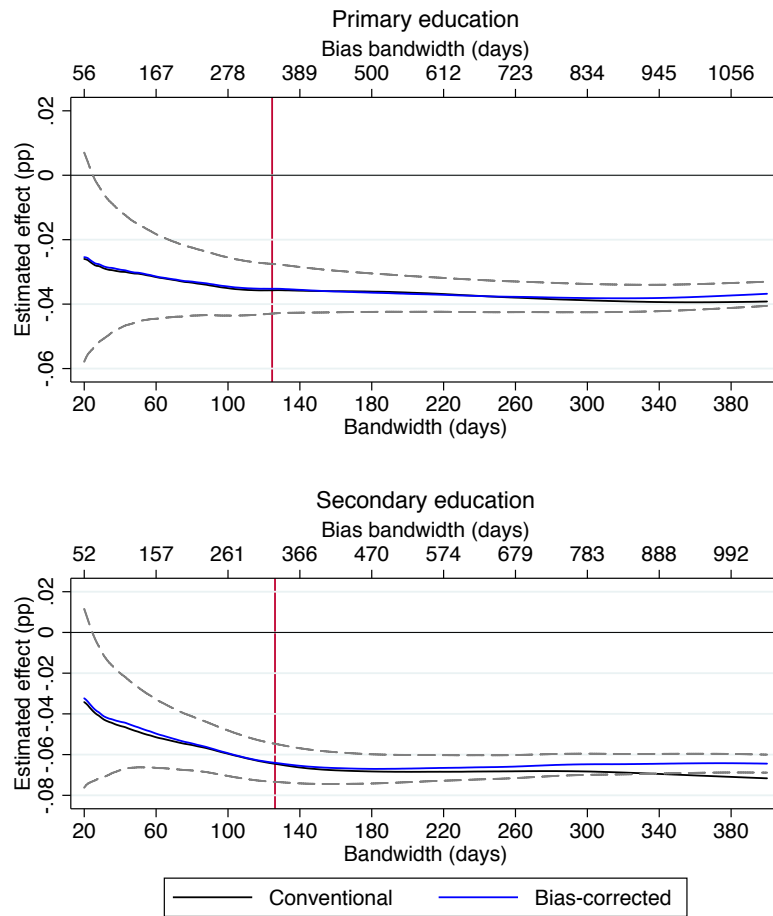
Variable	Sharp RD estim.	CER-optimal bw.	Bias bw.	p-value
At 18+ threshold in 2016				
Primary education	0.004	75.74	179.3	0.354
Secondary education	0.003	45.37	158.5	0.450
Females	0.006	117.1	269.9	0.004
Biometric registration	-0.006	54.06	158.1	0.051
At 18+ threshold in 2014				
Primary education	-0.036	124.6	346.2	0.000
Secondary education	-0.064	126.2	329.4	0.000
Females	0.001	144.9	328.0	0.740
Biometric registration	-0.032	92.72	287.7	0.000

Note: Sharp RD estimator, CER optimal bandwidth, bias bandwidth and robust p-values are shown. Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the CER-optimal bandwidth is chosen automatically to minimise the coverage error rate of the robust bias-corrected confidence interval. Predetermined covariates are aggregated to date of birth.

One explanation for the imbalances in the education variables is that they are an artefact of comparing younger and older citizens across relatively large (although CER-optimal) bandwidths of around 125 days. To examine how sensitive the imbalances at the 18+ threshold in 2014 are to alternative bandwidths, I therefore replicate the balance tests for the primary and secondary education variables using all bandwidths between 20 and 400 days (see Figure C4). The results suggest that the two education variables only become balanced at relatively small bandwidths of 24 days. For all bandwidths smaller than 24 days we cannot reject the null of no discontinuity using 95% confidence intervals.

Next, I examine whether the main RD results on the downstream turnout effect of compulsory voting hold when using only observations within the narrow bandwidth of 24 days (where the education variables are balanced). The small number of mass points around the threshold (24 days on each side) means that a continuity-based RD analysis is not advisable and a local randomisation RD approach is preferable (Cattaneo, Idrobo and Titiunik, 2018b). The local randomisation RD approach makes the (stronger) assumption that there is a

Fig. C4 Education balance at alternative bandwidths – 2016 data



Note: Note: The figure displays the estimated effect (for various bandwidths) of compulsory voting eligibility in 2014 on the proportion of individuals (who share the same birthdate) with completed primary (or secondary) education in 2016. Conventional (solid black line) and bias-corrected (solid blue line) point estimates from local linear regressions with triangular kernel; the vertical red line marks the CER-optimal bandwidth; dashed lines show robust bias-corrected 95% confidence intervals.

narrow window around the voting age threshold where observations are assigned to treatment and control group as in a randomised experiment (Lee, 2008). In the local randomisation framework, treatment effects can be estimated using simple difference-in-means tests rather than local polynomial regressions (Cattaneo, Idrobo and Titiunik, 2018b). To guarantee sufficient statistical power, the outcome variable is a binary indicator of individual-level turnout in the 2016 municipal election, rather than the average turnout by birthdate measure used in the main analysis.

Overall, the main results are confirmed when using the local randomisation RD approach and the 24 days bandwidth. Results from a t-test and a chi-squared test suggest that 2016 turnout is on average 1 percentage point *lower* amongst individuals in the treatment group ($n = 215,507$), who were already eligible for compulsory voting in 2014, compared to individuals in the control group ($n = 188,248$), who are first-time compulsory voting eligibles. These results are effectively the same as the main RD estimates. Similar results are also obtained from a logistic regression using 2014 compulsory voting eligibility as an indicator to predict 2016 turnout. These results are robust to clustering the standard errors by birthdate. Furthermore, disaggregated RD analysis based on citizens' educational status suggests that the main RD results hold when conditioning on individuals' educational background (see Appendix G).

Another common response to covariate imbalance in RD designs is to include the “problematic” covariate as a control variable (Calonico et al., 2019). If the RD estimate is robust to covariate-adjustment, this is typically interpreted as evidence in favour of the identifying assumption of continuous potential outcomes at the threshold.⁴³ Table C6 below presents results from sharp RD models predicting the effect of compulsory voting on downstream turnout whilst controlling for primary and secondary education respectively. For consistency, the models use the same bandwidths as the main RD model used for predicting downstream turnout (see Model 1 in Table 4.1). In order to control for educational attainment at the individual level, the models are fitted on the *raw* 2016 turnout data instead of the data collapsed by date of birth.⁴⁴

Overall, controlling for educational attainment does not appear to change the main findings. In Model 1 (which controls for completed primary education) the estimated effect of CV on downstream turnout is negative and statistically significant as in the main results. In Model 2 (which controls for completed secondary education) the estimated effect of CV on downstream turnout is also negative and statistically significant as in the main results.

⁴³Calonico et al. (2019) show that, whilst this approach can improve the precision of RD estimate, it also involves invoking parametric assumptions on the regression functions, or redefining the parameter of interest.

⁴⁴Cattaneo, Idrobo and Titiunik (2018b) show that when the running variable is discrete and the number of mass points is relatively large (as is the case here), this is equivalent to fitting the RD model on the collapsed data.

Table C6 Effect of compulsory voting on downstream turnout controlling for education – 2016 data

	(1) Control: primary education	(2) Control: secondary education
Treatment effect	-0.005	-0.004
Robust p-value	(0.000)	(0.000)
Observations	20,634,342	20,634,342
Bias bandwidth	203.4	203.4
Bandwidth	118.6	118.6
Eff. nr. obs. R	1,020,746	1,020,746
Eff. nr. obs. L	913,173	913,173
Robust 95% CI R	-0.00313	-0.00258
Robust 95% CI L	-0.00831	-0.00777

Note: Sharp RD models predicting the effect of 2014 eligibility for compulsory voting on 2016 turnout. Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the bandwidths are the same as in the main RD model predicting downstream turnout (see Model 1 in Table 4.1). Primary education is a binary indicator of whether an individual completed primary education at the time of registration. Secondary education is a binary indicator of whether an individual completed secondary education at the time of registration. The model is fitted on the raw data from the 2016 voter file.

Placebo cut-off tests

Another falsification test frequently employed in RD analysis examines whether there are significant treatment effects on the outcome of interest (mean turnout by birthdate) at placebo cut-offs away from the true cut-off. The presence of significant discontinuities away from the true cut-off would cast doubts on the appropriateness of the RD specification (Hyytinen et al., 2018). Using data from the 2016 municipal election, Table C7 presents sharp RD estimates for several placebo cut-offs (in increments of 100 days), using local linear regressions and MSE-optimal bandwidths. There is no evidence of significant discontinuities away from the true cut-off at age 18 in 2014, and only one significant discontinuity away from the true cut-off at age 18 in 2016. These null findings suggest that the main RD specification used in the paper is appropriate.

Table C7 Sharp RD estimates for placebo cut-offs – 2016 data

Alternative cut-off	Sharp RD estimator	Robust p-value	MSE-optimal bandwidth	Bias bandwidth	Effective Obs. (left)	Effective Obs. (right)
18+ in 2016						
-300	-.00452	0.001	67.7	131.4	67	68
-200	-.00158	0.450	62.2	91.5	62	63
-100	-.00214	0.422	53.2	78.7	53	54
0	.05222	0.000	77.5	163.2	77	78
100	.00173	0.910	114.5	203.4	100	115
200	.00104	0.921	55.6	118.6	55	56
300	.00259	0.035	46.8	86.3	46	47
18+ in 2014						
-300	.00025	0.733	97.2	149.3	97	98
-200	-.00023	0.974	63.7	88.5	63	64
-100	.00324	0.599	103.7	124.7	103	100
0	-.00874	0.000	119.1	204.0	119	120
100	.00037	0.840	100.4	135.9	100	101
200	.00086	0.383	102.4	150.1	102	103
300	-.00169	0.243	103.2	158.5	103	104

Note: Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the MSE-optimal bandwidth minimises the mean squared error of the local polynomial point estimator. Observations used for placebo estimates are restricted to those on the same side of the true cut-off. Estimation with observations from the opposite side would be invalid due to the actual non-zero treatment effect observed at the true cut-off.

Sharp RD estimates using raw turnout data

As an additional robustness check, I re-run the main RD analysis on the raw 2016 turnout data instead of the collapsed data – both with and without clustering the standard errors at the birth date-level. The clustering approach is recommended by [Lee \(2008\)](#) for RD designs where the running variable is discrete. [Kolesár and Rothe \(2018\)](#), however, show that when the number of mass points is moderate, the clustering approach can lead to confidence intervals with substantially worse coverage properties than those based on conventional standard errors. Regardless, [Cattaneo, Idrobo and Titiunik \(2018a\)](#) show that when the running variable is discrete and the number of mass points is relatively large (as is the case here), both approaches are roughly equivalent to fitting the RD model on the collapsed data. The results presented in [Table C8](#) confirm this view. The estimated treatment effects of compulsory voting eligibility on contemporaneous and downstream turnout are very similar to the main results.

Table C8 Sharp RD estimates using raw 2016 turnout data

	(1)	(2)	(3)	(4)
	Contemporaneous		Downstream	
Treatment effect	0.053	0.053	-0.008	-0.008
Robust p-value	(0.000)	(0.000)	(0.000)	(0.000)
Observations	20,634,342	20,634,342	20,634,342	20,634,342
Bias bandwidth	168.3	158.8	197.4	211.5
Bandwidth	72.90	69.76	119.2	116.1
Eff. nr. obs. R	420385	402300	1.029e+06	1.004e+06
Eff. nr. obs. L	325245	313495	920462	896535
Robust 95% CI R	0.0558	0.0555	-0.00622	-0.00573
Robust 95% CI L	0.0492	0.0496	-0.0115	-0.0122
Clustered SEs		✓		✓

Note: The binary dependent variable is individual-level turnout. Point estimates are from local linear regressions with triangular kernel and MSE-optimal bandwidths. Robust 95% confidence intervals are bias-corrected. In Models (2) and (4) the standard errors are clustered at the birthdate-level.

D Estimating the voting age population

One way to address the potential problem of differential registration rates amongst treatment and control group at the compulsory voting age threshold (see Appendix C) is to adjust estimates of the treatment effect by the registration rate for each group, which can be approximated using external data on daily births in Brazil (Cepaluni and Hidalgo, 2016). This strategy amounts to replicating the main RD analysis using estimated daily birth totals as the denominator for calculating turnout rates (T^{Pop}) rather than using the number of registrants as the denominator (T^{Reg}) (Nyhan et al., 2017). Ideally, the denominator would be the voting-eligible population (VEP) or the voting-age population (VAP), but lack of data means that we need to rely on daily birth totals as a proxy (Cepaluni and Hidalgo, 2016). Daily birth totals are more likely to approximate the VAP than the VEP, given that disenfranchised populations such as non-citizens, conscripts and prisoners are included (Nyhan et al., 2017).

Data on the daily number of births come from the *Sistema de Informações sobre Nascidos Vivos* (SINASC), which records all live births in Brazil since 1994.⁴⁵ SINASC was rolled-out gradually throughout the 1990s, which means that the data are likely to be incomplete for

⁴⁵The SINASC data and documentation are available at <http://www2.datasus.gov.br/DATASUS/index.php?area=0901> (accessed 16/03/20).

the relevant time-period. The turnout estimates based on birth totals in the denominator are therefore not valid estimates of T^{Pop} . However, the difference between the estimates of T^{Pop} in the treatment and control group is a valid estimate of the treatment effect that is unconditional on registration (Nyhan et al., 2017). The only assumption needed is that there is no systematic difference in the under-recording of live births in the SINASC data in the narrow window around the compulsory voting eligibility thresholds.

Table C9 below presents sharp RD estimates of the effect of compulsory voting eligibility on ‘contemporaneous’ turnout for elections between 2012 and 2016, using birth totals rather than registrants as the denominator for calculating turnout. Prior elections are not included given that corresponding SINASC birth data are not available for pre-1994 years. Model specifications and bandwidths are the same as in the main RD analysis (see Table C2) to allow for comparison between results that are conditional and unconditional on registration. In line with the main results, the estimated treatment effect of compulsory voting on contemporaneous turnout is consistently positive across elections. At around 0.05, the unconditional estimate for 2016 is remarkably similar to the conditional estimate for that election year. Unconditional estimates for 2012 and 2014 are larger than the corresponding conditional estimates, but point in the same direction. This indicates that the main results hold even when the possibility of differential registration rates between treatment and control group is accounted for.

Table C9 Unconditional RD estimates of contemporaneous turnout effect – 2012-2016

	(1) 2016 - Municipal	(2) 2014 - General	(3) 2012 - Municipal
Treatment effect	0.053	0.178	0.173
Robust p-value	(0.000)	(0.000)	(0.000)
Observations	2,465	1,738	1,010
Bias bandwidth	161.9	138.1	175.2
Bandwidth	76.80	50.89	73.50
Eff. nr. obs. R	77	51	74
Eff. nr. obs. L	76	50	73
Robust 95% CI R	0.0601	0.205	0.177
Robust 95% CI L	0.0479	0.148	0.141

Note: Sharp RD estimates are from local linear regressions with triangular kernel. The coefficient represents the estimated effect of being eligible for compulsory voting in one election on turnout in that same election unconditional on being registered. Bandwidths are fixed at the same values as Models 1-3 in Table C2 to allow for direct comparison between conditional and unconditional estimates.

Table C10 below presents sharp RD estimates of the effect of compulsory voting eligibility on ‘downstream’ turnout for elections 2014 and 2016, using birth totals rather than registrants as the denominator for calculating turnout. Prior elections are not included given that corresponding SINASC birth data are not available for pre-1994 years. Model specifications and bandwidths are the same as in the main RD analysis (see Table C3) to allow for comparison between results that are conditional and unconditional on registration. In line with the main results, the estimated treatment effect is negative across elections, although only statistically significant for the 2014 election. This provides indicative evidence that the main findings on downstream turnout hold even when differential registration rates between treatment and control group are accounted for.

Table C10 Unconditional RD estimates of downstream turnout effect – 2014-2016

	(1) 2016 - Municipal	(2) 2014 - General
Conventional	-0.000	-0.023
Robust p-value	(0.326)	(0.000)
Observations	2,465	1,738
Bias bandwidth	203.4	238.1
Bandwidth	118.6	140.7
Eff. nr. obs. R	119	141
Eff. nr. obs. L	118	140
Robust 95% CI R	0.00538	-0.0201
Robust 95% CI L	-0.0162	-0.0474

Note: Sharp RD estimates are from local linear regressions with triangular kernel. The coefficient represents the estimated effect of being eligible for compulsory voting in the previous election on turnout in the current election unconditional on being registered. Bandwidths are fixed at the same values as Models 1-2 in Table C3 to allow for direct comparison between conditional and unconditional estimates.

E Downstream turnout effects of voluntary voting

Given that elections in Brazil take place every two years at the beginning of October and voting is voluntary at age 16 and compulsory at age 18, there is a concern that the estimated effect of compulsory voting eligibility in one election (e.g. the 2014 general election) on turnout in a subsequent election (in this case the 2016 municipal election) may reflect a compound treatment effect that also includes the downstream turnout effect of becoming eligible for voluntary voting four years before (in the 2012 municipal election). This is a

legitimate concern given that several quasi-experimental studies from countries with voluntary voting rules have found evidence that eligibility for voluntary voting can have significant downstream turnout effects, even after several elections (Coppock and Green, 2016; Dinas, 2012; Meredith, 2009).

To address this concern, I examine whether eligibility for voluntary voting in the 2014 general election has any downstream effects on turnout in the 2016 municipal election. Given that there is a nearly perfect overlap between individuals who are barely eligible for voluntary voting in 2014 and individuals who are barely eligible for compulsory voting in 2016, the standard RD design used in the main analysis cannot be employed to assess the downstream turnout effect of voluntary voting. However, we can exploit the fact that the first round of Brazilian elections always takes place on the first Sunday of October, which means that the exact date of the first round varies slightly every two years. Because of this rule, there are some individuals (those born between 3 and 5 October 1998) who are eligible for voluntary voting *twice* (in 2014 and 2016) before becoming eligible for compulsory voting. These individuals can be regarded as the treatment group in this RD set-up. The control group instead includes individuals born between 6 and 8 October 1998 who are ineligible to vote in 2014 and only become eligible for voluntary voting in 2016. By comparing 2016 turnout amongst individuals within a narrow 3-day window around the voluntary voting eligibility threshold, we can therefore isolate the effect of voluntary voting eligibility in 2014 on turnout in 2016 without the risk of confounding the estimate with the contemporaneous turnout effect of compulsory voting.

Because including individuals who were born *before* 3 October 1998 would mean including individuals who are eligible for compulsory voting in 2016, it is not possible to vary the bandwidths around the eligibility threshold. The small number of mass points around the threshold (three days on each side) also means that a continuity-based RD analysis is not advisable and a local randomisation RD approach is preferable (Cattaneo, Idrobo and Titiunik, 2018b). To guarantee sufficient statistical power, the outcome variable is a binary indicator of individual-level turnout in the 2016 municipal election, rather than the average turnout by birthdate measure used in the main analysis. Results from a t-test and a chi-squared test suggest that there is no significant difference ($p = 0.16$) in 2016 turnout rates between treatment group ($n = 15,409$) and control group ($n = 16,340$). Similar results are obtained from a logistic regression using 2014 voluntary voting eligibility as an indicator to predict 2016 turnout ($p = 0.16$). Results also remain insignificant at the 95% confidence threshold when clustering the standard errors by birthdate ($p = 0.07$).

F Does mobilisation explain the first-time boost?

To examine the potential role of mobilisation as a mechanism behind the first-time compulsory voting boost, I disaggregate the main RD analysis of downstream turnout in the 2016 municipal election. Specifically, I examine whether the first-time compulsory voting boost is stronger in electorally competitive municipalities compared to uncompetitive municipalities, as we might expect if political parties focus their mobilisation efforts on marginal seats (Cox and Munger, 1989). In Brazil, municipal elections for mayor and municipal councillors are held every four years. These elections are dominated by the contest for mayor (Klašnja and Titiunik, 2017), which in most cases follows a single-ballot plurality rule.⁴⁶

To establish the electoral competitiveness of a municipality, I use several measures of the margin of victory in mayoral elections.⁴⁷ First, the winner's percentage of the vote in the 2016 mayoral election minus the runner-up's percentage of the vote ("percentage winning margin"). Second, the average percentage margin of victory in all mayoral elections between 2004 and 2012, which is an *ex ante* measure of competitiveness and therefore a more suitable indicator of the expected closeness of the 2016 mayoral election ("expected closeness").⁴⁸ Municipalities with a percentage winning margin or expected closeness of equal to or less than 10 percentage points are considered competitive, whilst all other municipalities are considered uncompetitive. Third, the total number of votes for the winner minus the total number of votes for the runner-up in the 2016 mayoral election ("raw winning margin"). Using the raw winning margin is less likely to result in spurious correlation between the margin of victory and turnout, as it does not include total votes cast in the denominator, which by construction appears in the numerator of turnout (Cox and Munger, 1989). Municipalities with a raw winning margin of less than or equal to the median raw winning margin (806 votes) are considered competitive, whilst all other municipalities are considered uncompetitive.

In line with our expectations, the results from the disaggregated RD analysis indicate that the first-time compulsory voting boost is more pronounced in competitive municipalities compared to uncompetitive municipalities, regardless of the specific measure used to divide the sample into competitive and uncompetitive municipalities (see Tables C11-C13). Whilst the estimated treatment effect of compulsory voting on downstream turnout ranges from

⁴⁶In the 2% of municipalities that have 200,000 or more registered voters, elections for mayor follow a dual-ballot plurality rule, where a runoff is required between the top two candidates if no candidate gets an absolute majority in the first round.

⁴⁷Data on municipal election results come from the TSE data repository (available at: <http://www.tse.jus.br/eleitor-e-eleicoes/estatisticas/repositorio-de-dados-eleitorais-1/repositorio-de-dados-eleitorais>), were downloaded on 29/10/18 and pre-processed using the R-package "electionsBR" (available at: <https://cran.r-project.org/web/packages/electionsBR/vignettes/introduction.html>).

⁴⁸In the case of a run-off election, the margin of victory in the second round is used.

-0.6 to -0.7 percentage points in uncompetitive municipalities, this negative effect roughly doubles in size in competitive municipalities, ranging from -1.2 to -1.7 percentage points. The difference in point estimates between the sub-samples is statistically significant at the 95% confidence level for two competitiveness measures (“expected closeness” and “raw winning margin”) and just short of conventional significance thresholds for one of the competitiveness measures (“percentage winning margin”). The results provide indicative evidence that mobilisation by political campaigns may play a role in explaining the first-time compulsory boost in Brazil.

Table C11 Percentage winning margin – disaggregated RD estimates

2016 winning margin ≤ 10 pp	(1) Competitive	(2) Uncompetitive	(3) Difference
Treatment effect	-0.012	-0.007	0.005
Robust standard error	0.00207	0.00208	0.00294
(Robust) p-value	(0.000)	(0.001)	>0.05
Observations	2,920	2,920	
Bias bandwidth	263.9	210.8	
Bandwidth	135.8	121.2	
Eff. nr. obs. R	136	122	
Eff. nr. obs. L	135	121	
Robust 95% CI R	-0.00885	-0.00315	0.01076
Robust 95% CI L	-0.0170	-0.0113	-0.00076

Note: The outcome variable is average turnout by birthdate in the first round of the 2016 municipal election. Municipalities with a percentage winning margin of equal to or less than 10 percentage points in the 2016 mayoral election are considered competitive and all other municipalities are considered uncompetitive. Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; MSE-optimal bandwidths are chosen in a data-driven, automatic way that seeks to minimise the mean squared error of the local polynomial point estimator. To compute the standard error and confidence interval around the difference between the subsamples, the sum of variances of the two point estimates was used as an estimate of the variance of the difference

Table C12 Average percentage winning margin – disaggregated RD estimates

2004-2012 winning margin ≤ 10 pp	(1) Competitive	(2) Uncompetitive	(3) Difference
Treatment effect	-0.014	-0.007	0.007
Robust standard error	0.00255	0.00185	0.00339
(Robust) p-value	(0.000)	(0.000)	<0.05
Observations	2,920	2,920	
Bias bandwidth	333.2	204.2	
Bandwidth	165.8	118.3	
Eff. nr. obs. R	166	119	
Eff. nr. obs. L	165	118	
Robust 95% CI R	-0.0103	-0.00391	0.01364
Robust 95% CI L	-0.0202	-0.0112	0.00034

Note: The outcome variable is average turnout by birthdate in the first round of the 2016 municipal election. Municipalities with an average percentage winning margin of equal to or less than 10 percentage points for mayoral elections 2004-2012 are considered competitive and all other municipalities are considered uncompetitive. Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; MSE-optimal bandwidths are chosen in a data-driven, automatic way that seeks to minimise the mean squared error of the local polynomial point estimator. To compute the standard error and confidence interval around the difference between the subsamples, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

Table C13 Raw winning margin – disaggregated RD estimates

2016 raw margin \leq median raw margin	(1) Competitive	(2) Uncompetitive	(3) Difference
Treatment effect	-0.018	-0.007	0.011
Robust standard error	0.00243	0.00173	0.00342
(Robust) p-value	(0.000)	(0.000)	<0.05
Observations	2,920	2,920	
Bias bandwidth	350.3	202.1	
Bandwidth	203	120.5	
Eff. nr. obs. R	204	121	
Eff. nr. obs. L	203	120	
Robust 95% CI R	-0.0135	-0.00377	0.01771
Robust 95% CI L	-0.0231	-0.0105	0.00428

Note: The outcome variable is average turnout by birthdate in the first round of the 2016 municipal election. Municipalities with a raw winning margin of less than or equal to the median raw winning margin (806 votes) are considered competitive and all other municipalities are considered uncompetitive. Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; MSE-optimal bandwidths are chosen in a data-driven, automatic way that seeks to minimise the mean squared error of the local polynomial point estimator. To compute the standard error and confidence interval around the difference between the subsamples, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

G Who is mobilised?

To assess whether the introduction of compulsory voting at age 18 effects downstream turnout asymmetrically across different socio-demographic groups, I replicate the main RD analysis on sub-samples of the voter file based on individuals' educational status and gender. Educational status is an acceptable proxy for socio-economic status in the context of Brazil (Cepaluni and Hidalgo, 2016). The voter file records individuals' self-reported highest level of education at the time of registration. Table C14 lists the eight different education categories recorded in the voter files. To avoid measurement error for those who continued their education after registration, the education variable is coarsened into a binary indicator of whether an individual completed primary education at the time of registration. By age 16, which is the earliest age at which one can register to vote in Brazil, primary education completion should be accurately measured (Cepaluni and Hidalgo, 2016).

Table C14 Education categories recorded in voter file

	Educational status	Grau instrução
1	Illiterate	Analfabeto
2	Can read and write	Le e escreve
3	Incomplete primary education	Ensino fundamental incompleto
4	Complete primary education	Ensino fundamental completo
5	Incomplete secondary education	Ensino médio incompleto
6	Complete secondary education	Ensino médio completo
7	Incomplete tertiary education	Superior incompleto
8	Complete tertiary education	Superior completo

Note: Educational status in the voter file is self-reported by citizens at the time of their first registration.

Table C15 shows that amongst all voters registered for the 2016 election, the more educated turned out at a significantly higher rate than the less educated, regardless of whether they were eligible for compulsory voting. The difference in average turnout rates between the less and more educated is particularly pronounced amongst those eligible for voluntary voting.

Table C15 2016 turnout rate (%) by educational status

	Less educated	More educated	Difference (pp)
Eligible for comp. voting	83.2	84.6	-1.4*
Eligible for vol. voting	42.5	62.9	-20.4*

Note: * $p < 0.01$, Chi-squared test. The sample includes all literate voters registered for the 2016 municipal election. Less educated individuals have incomplete primary education or no formal education at the time of registration. More educated individuals have completed at least primary education at the time of registration.

Next, I examine whether compulsory voting affects downstream turnout asymmetrically depending on citizens' educational status. Table C16 below presents sharp RD estimates from local linear regressions fitted separately on sub-samples of more and less educated individuals in the 2016 voter file. The results suggest that the first-time compulsory voting boost is more pronounced amongst the more educated than amongst the less educated. However, the *difference* in point estimates between the sub-samples does not reach the conventional significance threshold, so we cannot reject the null hypothesis of no difference between the two point estimates. The insignificant RD estimate for the less educated sub-sample may indicate that downstream turnout behaviour amongst this group remains largely unaffected by the introduction of compulsory voting at age 18. Finally, I examine whether compulsory voting affects downstream turnout asymmetrically depending on citizens' gender. Table C17 below presents sharp RD estimates from local linear regressions fitted separately on sub-samples of men and women in the 2016 voter file. The results suggest that the first-time compulsory voting boost is more pronounced amongst men than amongst women. In this case, the *difference* in point estimates between the sub-samples is statistically significant at the 95% confidence level.

Table C16 Effect of compulsory voting on downstream turnout by education – 2016 data

	(1) More educated	(2) Less educated	(3) Difference
Treatment effect	-0.006	-0.001	0.005
Robust standard error	0.0014	0.0034	0.0038
(Robust) p-value	(0.000)	(0.562)	>0.05
Observations	2,920	2,920	
Bias bandwidth	197.8	284.9	
Bandwidth	123.1	137.2	
Eff. nr. obs. R	124	138	
Eff. nr. obs. L	123	137	
Robust 95% CI R	-0.00361	0.00462	0.0126
Robust 95% CI L	-0.00931	-0.00851	-0.0026

Note: Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the MSE-optimal bandwidth minimises the mean squared error of the local polynomial point estimator. More educated individuals have completed primary education at time of registration. Less educated individuals have incomplete primary education at time of registration. To compute the standard error and confidence interval around the difference between the subsamples, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

Table C17 Effect of compulsory voting on downstream turnout by gender – 2016 data

	(1) Men	(2) Women	(3) Difference
Treatment effect	-0.011	-0.005	0.006
Robust standard error	0.00220	0.00160	0.00266
(Robust) p-value	(0.000)	(0.001)	<0.05
Observations	2,920	2,920	
Bias bandwidth	190.7	300.1	
Bandwidth	127.2	194.3	
Eff. nr. obs. R	128	195	
Eff. nr. obs. L	127	194	
Robust 95% CI R	-0.00657	-0.00213	0.01121
Robust 95% CI L	-0.0152	-0.00839	0.00078

Note: Point estimates are constructed using local polynomial estimators with triangular kernel; robust p-values are constructed using bias-correction with robust standard errors; the MSE-optimal bandwidth is chosen in a data-driven, automatic way that seeks to minimise the mean squared error of the local polynomial point estimator. To compute the standard error and confidence interval around the difference between the subsamples, the sum of variances of the two point estimates was used as an estimate of the variance of the difference.

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